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A native Maori and his wife sluicing for gum  
GATHERING KAURI GUM IN NEW ZEALAND [See page 360]

# The Pleistocene Man of Vero, Florida

## A Summary of the Evidence of Man's Antiquity in the New World

By F. H. Sterns

AFTER a scientific controversy has continued for some time, when the greater portion of the evidence has probably been presented, it appears advisable that some spectator, removed from the heat of the fray, should summarize the contentions of the different scientists, and point out what conclusions a disinterested student would draw from the presentations of the various parties to the dispute. By this means, the different writers can see where they have failed to make clear their side of the case, where their logic may be faulty, and where their data seems insufficient for their conclusion; and thus they may be led to abandon false positions, or to clear away honest doubts if their theories are sound.

In the case of the discovery of the fossil man of Vero, the writer will try thus to summarize the evidence. He will also endeavor to point out certain logical fallacies, which reappear in this controversy, just as they have done in the discussion of every supposed case of Pleistocene man in America. Further there will be an attempt to add certain new evidence from another field, which bears on some phases of the discussion.

Mr. E. H. Sellards, State geologist of Florida, has reported from Vero, Fla., the finding of human remains consisting of skeletal material, flint flakes, bone implements, and fragments of pottery, in intimate association with fossils of extinct vertebrates. He states that both the human remains and the fossils were included in the formation by primary deposition, and hence were of the same age. By a study of the fauna, he arrives at the conclusion that they belong to the Pleistocene period.

Now the assignment of a relatively highly developed man to the Pleistocene period is not in itself startling, as the whole Upper Paleolithic period in Europe with its high artistic development belongs to the glacial epoch, while *Pithecanthropus erectus* may well be upper Pliocene. Further, from the study of the fossil anthropoids, many writers have reached the conclusion that man originated in the Miocene.

Nor is there on *a priori* grounds any reason for doubting that man had arrived on the American continent by glacial times. The close of the Pleistocene in America, according to many authorities, dates back only from eight to fifteen thousand years. That comparatively short period is not too much to expect for the development of the indigenous cultures in America or its diversification. The ninth cycle of the Maya calendar, when those people attained the height of their civilization in Central America, was contemporaneous with the early Christian era, while the first cycle would be thousands of years earlier. Likewise for the linguistic and somatological variation on the continent long periods of time would be expected.

The earliest fossil remains of man in Europe seem to represent races not in the direct line of human evolution. The non-existence as far as is known of any of the fossil anthropoids or any of man's nearest relatives among the primates in the New World seems to preclude the possibility that the human species had its origin in the Americas. The best available evidence is for an Asiatic source for humanity.

If this be true, America may have been peopled by man before Europe was, and the arts and cultures of America at certain times may have been well in advance of those of the Old World during the same period. At any rate, until we have evidence to the contrary, no negative conclusion based upon similarity of physical type or of cultural status with the modern inhabitants of the New World or of the diversity of these from a standard of antiquity derived from Europe is sound. Until we know what Pleistocene man in America looked like and how he acted, if he existed, we have no right to judge an individual find by the supposed nature of the human race then. We have no right to approach the question with a theory. The evidence for or against man's antiquity must be geological, and not cultural or somatological.

Nevertheless, although there seems to be no *a priori* reason for rejecting a find of Pleistocene man in America, such discoveries have always been doubted. There is a certain school of scientists in this country which is violently opposed to the acceptance of any conclusion which even suggests a great antiquity of man on this continent. The members of this school are of such prominence that faulty arguments on their part seem to carry more weight than the soundest arguments on the parts of others. Thus almost every find of so-called Pleistocene man has been discredited without its supporters having received a fair hearing. Therefore such a discovery is vastly important because of its possible

bearing on the much disputed question of man's antiquity in the New World. It is also interesting because of the almost certain controversy it will arouse.

When Mr. Sellards found where his evidence was leading him, he realized the desirability of having other scientists study the site. So he invited, at one time or another, several geologists and archeologists to make supplementary investigations at Vero. Among those who responded to this invitation were Rollin T. Chamberlain, George Grant MacCurdy, Ales Hrdlicka, Edward W. Berry, Oliver P. Hay, R. W. Shufeldt, and T. W. Vaughan. The first three of these reached negative results while the others were generally confirmatory. All have published papers, as will be seen by the bibliography at the close of this article.

As the question at issue is primarily one of geology, it seems best to study the geological features first. Fortunately there seems to be a substantial agreement in regard to the geological section at Vero. In the vicinity of the finds, this section is as follows:

3. Upper creek deposit.
2. Lower creek deposit.
1. Marine shell marl.

The lowest member of this series is largely a marine deposit, but it grades in its upper portions into beach and eolian sands. It is prevalently a shell marl, in which one can easily find molluscan fossils; but it contains many beds of sand, especially near the top. It is known to extend both north and south of Vero several hundred miles along the Atlantic seaboard. With the exception of a portion of a camel's humerus found some distance away in a formation possibly identical, the entire known fossil content consists of marine molluscs, all of which belong to species now living in the adjacent ocean. All the observers agree that wherever found this formation can be readily identified, and since it underlies the whole area under discussion it forms a definite and undisputed datum plane of reference.

In spite of the absence of any extinct species or even of species which have changed their habitat, every one of the investigators admits that this formation is of Pleistocene age, though the grounds for this assignment are not made clear. Unless they concede that layer No. 2 is either Pleistocene or immediately post-Pleistocene, it seems to me that their conclusion is hasty, and unwarranted by any facts they have revealed. It is only fair to state that the writers do not agree upon the part of the Pleistocene to which it belongs—Berry being sure that it is late Pleistocene, while Hay implies that it is early and the other writers are less explicit.

There seems to be no dispute in regard to the existence of a partial unconformity between layers No. 1 and No. 2. In places, the older beds have been considerably eroded, with the formation of depressions and pockets in their surface. At other places, however, there is no distinct line of separation between these strata, although they can always be distinguished by their fossil content. The virtual agreement of the observers that there was no great hiatus between beds No. 2 and No. 3 seems to carry with it the conclusion that the time separation of No. 1 and No. 2 also was not great, although in both cases there was a gradual change in the conditions of deposition.

Formation No. 2 is composed of coarse sand at the bottom which grades upward into fresh water marl. In the low places and holes of formation No. 1, it consists of a muck which includes wood, sticks, acorns, small shells, and vertebrate fossils. Near the bottom, it is distinctly cross-bedded and light colored; but towards the top, it loses its cross-bedding, and becomes dark by the inclusion of organic remains.

Its age is in dispute. Of the considerable number of animal and plant forms it contains, many of the species are still living in the Vero region while others are now extinct. Of the 27 fossil plants, 19 have not previously been reported in the fossil form, while the remaining 8 have been found before in Pliocene or Pleistocene formations. One is an entirely extinct species—even the genus of which is now represented by a single species of the arid region of Texas and Arizona. About seventy per cent of the mammalian remains are extinct forms. Most of these correspond to species found in the Sheridan beds of eastern Wyoming and western Nebraska and the Aftonian (first interglacial) sands further east. Their occurrence later than the middle Pleistocene is not known.

However, it has been urged that although the fauna

belongs to the early or middle Pleistocene in other regions, yet in the milder environment of Florida it might have persisted longer. Personally I wonder why it should have perished at all in Florida, as the climate would continue to grow milder as the ice retreated from the Ohio valley. The trouble with this argument is that it proves too much. Hay has pointed out its essential absurdity by asking why a mixed fauna, partly native and partly derived from northeastern Asia and South America, should survive unchanged several severe alternations of glacial and interglacial climate and then suddenly become extinct within recent times. It is possible that they did survive a little longer in the south than they did in the north; but the very argument for this survival presupposes a climate, becoming progressively more arctic (reaching Florida, of course, last), such as would characterize the advance of an ice sheet. Therefore the very reasons advanced for the continuation of this fauna beyond middle Pleistocene times demands their extinction before the close of the last glacial advance.

One further argument has been advanced against the Pleistocene age of bed No. 2. Chamberlain's contention is that the fossils represent a secondary deposition and hence are older than is the strata in which they are included. He claims that the bones of single individuals are very much scattered—more so than are those human remains which he acknowledges are in the bed by primary inclusion. Further he found in the bog formation of the uplands the source from which these fossils were derived. In substantiation of this idea, he tells of numerous examples occurring in bed No. 2 of "cannon balls" up to five inches in diameter of indurated black sandstone derived from the bog formation.

His position, however, has been shown to be untenable. In the first place, as he himself has since admitted the bog cannot be the source of the fossils, as persistent examination of it has failed to yield any animal remains, although these are abundant in bed No. 2. Furthermore, the remains in that bed are so fragile, but at the same time so well preserved, that they could not have been carried to their present position by the same process which rolled the blocks of indurated sandstone into "cannon balls." Moreover, the bones of many of the animals are less scattered than are those of the men. Finally the bones show little or no signs of water erosion. Thus it appears proven that the fossils are of the same age as the bed containing them, and that they indicate a Pleistocene age for it.

The partial unconformity between layers No. 2 and No. 3 is not so clearly marked as that between No. 1 and No. 2, and here is to be found, as will be shown later, the weakest point in the arguments of the supporters of the Vero man. Sellards, himself, has found it non-existent in places, although in other places it was clearly marked. In one section, he first assigned certain strata to bed No. 2, but he later considered it to be bed No. 3—an uncertainty not liable to occur if there was a definite unconformity present. Chamberlain, likewise, found the line of separation of the two beds easily on his first visit, while on his second visit he tells of a great confusion in distinguishing between the two layers. At the time of his second visit to the site, he doubted whether the beds could properly be separated from each other at all. On this point, I wish that the various observers would give us more light, as it appears to be an important matter to the reader of the evidence.

No. 3 is an alluvial bed, containing fossils which do not differ greatly from those of bed No. 2 although the proportion of extinct species is less. Chamberlain insists that this bed, at least, is reworked, and hence is younger than its fossil content. He would derive its fossils from bed No. 2. In support of this claim, he mentions that the bones of single individuals are more scattered in No. 3 than they are in No. 2. Sellards, on the other hand, shows that certain extinct turtles are represented by almost complete shells in the bed under dispute, and certain fossil birds of extinct species occur only in it. I am convinced that Pleistocene age has been proved for bed No. 2, but for No. 3 the evidence appears inconclusive. It seems that the upper portion of this bed must be recent, although it is probable that its lower portions may be Pleistocene.

We will now discuss the remains of man as related to these beds. In bed No. 2 were found parts of at least two skeletons. There has also been discovered some flint flakes, a broken portion of a bone implement, and some bones of animals marked supposedly by human tools. In strata No. 3, there has been found bone and

flint implements, an abundance of pottery, and portions of several skeletons. Unfortunately the bones of bed No. 2 seem to have occurred near the contact between the two beds, if they do not belong to both beds. This seems to me to present a great difficulty. If the dividing line between the beds is not certain (a point which I have shown before to be still unsettled), then we may not be sure these remains belong to the bed to which they were assigned. If the bones are of the age of the deposit, this confusion makes considerable difference in the possible interpretations of the facts. If the bones belong to one bed, they are certainly of Pleistocene age, while if they belong to the other, they are only probably of that age.

Here one might say that Sellards found the dividing line "distinct and unmistakable" and Chamberlain said that the bed No. 2 "contains human bones essentially *in situ* beyond reasonable doubt." It was only after it was definitely proved that this bed was of Pleistocene age that there was any doubt as to its line of separation with bed No. 3. In other words, it was the apparent clash of the facts with previously held theories which lead to the doubts. These theories and presuppositions will be discussed later in this paper.

The remaining evidence of the presence of man during the formation of bed No. 2 is less conclusive. The flint flakes may be intrusive from the layer above. Moreover although they probably have been chipped by man, they do not certainly belong to the class of humanly made artifacts. The single fragment of a bone needle may likewise have been an intrusive from the upper bed, where such bone implements are numerous. Furthermore the scratches on certain bones which have been considered as the work of man may not be such.

However, if these are rejected the case for man belonging to bed No. 2, is strengthened rather than weakened. For in that case, the difficulties in the question of the line of separation of beds No. 2 and No. 3 vanish. For we have a definitely marked cultural stratification. Then all the flints, all the pottery fragments, and all the bone implements belong to the upper layer. It is then definitely marked off from the lower layer, and so we would expect observers to distinguish the two beds by their contents if they were unable to do so by their appearance or the presence of an unconformity. Moreover one of the arguments of the anthropologists against the find is no longer valid. They have objected that the presence of pottery shows too high a civilization for middle Pleistocene man. If pottery and all other artifacts of a superior order occur only in the upper layers, this argument is of no effect. Lastly if the abundance of pottery and flint implements in the upper layer can exist without the intrusion of a single specimen or only a few specimens into the lower layer, then the probability of the larger bones of the human skeleton being thus intruded by natural means is very small.

We now come to the question of intrusion by human agency. It has been claimed, however, without evidence that the skeletons in bed No. 2 represent burials. It seems to me that the supporters of the finds have definitely disproved this hypothesis. They have shown that there was a definite stratification above the remains and that this showed no signs of disturbance such as would be necessary in case of a burial. Furthermore certain fossil plants were discovered above the human remains, and these likewise were undisturbed.

We come now to certain implied arguments, drawn from generally held theories, but of which the legitimacy is open to grave doubts. In the first place, it has been held that the cultural status was too high for Pleistocene man. Secondly it is claimed that the artifacts and bones resemble so closely the modern Indian of the vicinity that they cannot be very old. Both of these arguments are based on a preconceived theory of the nature Pleistocene man must have. There has been set up artificial standards to which the finds must conform, although these standards are based upon no evidence whatsoever. Until we know what the skeletal type of Pleistocene man in America was and the kind of implements he made, we have no right to base any argument upon type. If we do, we will notice that the Sussex man, for example, has many features similar to modern Englishmen, and the implements with him resemble some used by the modern Tasmanians, and therefore we must conclude that he is only a couple of hundred years old in spite of all the stratigraphic evidence to the contrary. Thus we must insist that all arguments advanced shall be geological rather than technological or somatological.

A further argument has been advanced that this represents an isolated case and therefore proves nothing. Hay has shown that by the same argument many other finds of glacial man have been repudiated, and that while one of them might be an isolated case, the whole group together are not so. It might be added further that all facts of science are isolated cases if you examine them separately and that if this argument is carried to its

logical conclusion, no science whatsoever is possible.

Hay has further pointed out that all the cases for glacial man in America so far discovered associate man with a fauna belonging to the Aftonian interglacial epoch. I may say that I have made extensive search in Aftonian beds myself. I have never found anything conclusively human in those beds although I have discovered many things which possibly show human workmanship.

In conclusion, I think we are justified in saying that the supporters of Pleistocene man at Vero have shown the probabilities to be in their favor although they have not absolutely proved their case.

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#### Some Reactions of Acetylene

By W. R. Hodgkinson, C.B.E.

It is well known that acetylene polymerises, mainly to benzene, when heated in a closed or partially closed vessel at 120°—150° C. At higher temperatures and pressure above normal, and especially at varying pressures, acetylene decomposes into carbon and hydrogen.

The gas can, however, be passed, at a moderate speed, through a glass or silica tube heated to 600°—800° C. either alone or diluted with an inert gas as nitrogen or hydrogen chloride or coal gas, for a moderate time (20—60 minutes) before any serious deposition of carbon occurs. The nature of the glass has a decided influence on the rate of decomposition. Silica has less effect than sodaglass.

Various metals were heated to temperatures from 100° to 1,060° C. in a stream of acetylene for like time periods. Cadmium, zinc, lead, tin, copper, aluminium, and others showed no particular action at temperatures up to their melting points. No change was found in the physical state of the metals, and no carbon was taken up by them, and the carbon deposited burnt off without residue.

Iron, nickel, cobalt, and to a lesser extent manganese, tungsten, platinum, and palladium, almost in this order, showed a decided reactivity when heated, in some cases

\*Temperatures were controlled by melting points of metals or by Rh-Pt pyrometer.

to very moderate temperatures (e. g., 200° C.), in acetylene, either pure or diluted.

Under favorable conditions of temperature and supply of the gas, the metal, if in moderately small pieces as wire, frequently glowed, the glow passing along the whole mass of metal employed. Carbon deposited on the metal and, after attaining a moderate thickness, the layer peeled off and another formed.

In the case of iron the strength of the metal was retained—in fact somewhat increased; in the case of nickel and cobalt (and to a lesser extent some of the metals before mentioned) a great physical change was observed. They became quite brittle, breaking up on attempting to bend them, and also showed very decided corrosion and pitting. This action (with nickel) commenced at comparatively low temperatures, e. g., 200° C., and became rapid at 600° C.

In the case of iron, a more or less deep carburization resulted. With nickel and cobalt carburization was also observed.

The most remarkable part of this action of acetylene is that as the carbon of the acetylene enters the metals (Fe, Ni, Co), some metal enters into and is contained in the soot-like deposit around the metal. This extrusion or volatilization of the metal was found to occur to a maximum extent with nickel, followed in order by cobalt and iron. A nickel wire (No. 14) was in fact entirely dispersed or disseminated into the sooty deposit by heating for a few hours in the gas stream.

The amount of metal contained in this black deposit was found to vary considerably in different experiments. The cause of variation is not yet quite determined.

In some cases as much as 15 per cent of nickel was obtained, in others about 5 per cent. With iron 3 per cent and more has been observed. Dilution of the acetylene was found not to stop the wandering of the metal, but simply to reduce the rate a little.

The carburizing of iron by heating in acetylene is very rapid, especially at temperatures above 800° C., and many observations indicate that it reaches a maximum just below 1,000° C. The type of carburization is quite peculiar, the carbon showing distinct diffusion into the metal. The accompanying photomicrographs are of iron carburized by heating in acetylene for different periods at 1,050° C.

Ammonia was used to dilute acetylene in some experiments on metals because it prevented to a remarkable degree the deposition of carbon from acetylene by heat, and also because the flame of the mixture was nearly smokeless.

In this gas mixture, iron, nickel and cobalt were found to be more rapidly carburized than by acetylene alone, and the amount of extraneous carbon, or soot, reduced.

It was imagined that the cause of the more rapid absorption of carbon by the metals might be due to the simultaneous formation of the nitride and possibly its further reaction with the acetylene.

Iron (or other metal) was heated in ammonia to 800° C. At something below this temperature fine iron wire was quickly converted into nitride, Fe<sub>3</sub>N.

Ferrous nitride heated in acetylene lost its nitrogen and the iron became highly carburized. Unfortunately, the physical change on conversion into nitride was not altered by the subsequent heating with acetylene. The iron remained brittle. On heating to bright redness and quenching, the steely structure of a highly carburized iron was obtained.

Further experiments showed that when ferrous nitride was heated in acetylene, ammonia was formed, but the matter was not followed up quantitatively.

Amines, as aniline, etc., have been found to act as very rapid carburizing agents on iron and nickel.

Many benzenic compounds (benzene, toluene, phenol, aniline, naphthalene, and others), when passed as vapor over nickel or iron, were found to act in a manner somewhat similar to acetylene—that is, a deposition of soot formed on the metal and a quantity of the metal was found to have wandered into this soot. In some cases the amount of nickel in the "soot" (benzene, aniline, phenol) was greater for the same time and temperature of heating than with acetylene.

Iron and other metals were observed to act in a similar manner, but not to the same degree as nickel.—*Jour. Soc. Chem. Ind.*

#### Tanning

CRYSTALLINE aromatic compounds containing a sulphonic or carboxylic group or both, soluble in water, and capable of precipitating glue and gelatin from solution, are used for tanning, instead of the amorphous materials hitherto used. Several examples are given. For instance, dihydroxyditolylmethane is treated with sulphuric acid, the sodium salt of the sulphonic acid produced is precipitated with common salt, and the aqueous solution of the sodium salt used for tanning in the ordinary way, after the addition of a little sulphuric acid.—Note in *Jour. Soc. Chem. Ind.* by J. L. JOHNSON.



Tree planter in operation

Planting trees with the original tree planter. This was drawn by horses, and was a great improvement over the old method of hand planting. Horse-power has now given way to gasoline



Cultivate the trees

The cultivation is done largely by disks. The orchard disk and the common disk are used alternately so as to keep the ground level. The one throws the soil in and the other throws it out

## Tree Planting to Control Snow and Wind\*

### Protection for the Farmer and the Railroad

By W. C. Palmer

FARMERS have for many years controlled snow about their farmsteads by tree planting and the railroads are now trying the same scheme in protecting their right of way. The common board panels that have been so commonly used are expensive, and in the winter with the big snow they often prove more of a detriment than a help. The trees can be planted so as to be effective under the most severe conditions and after they are planted and given a good start the cost of upkeep is slight.

The Minneapolis, St. Paul and Sault Ste. Marie (better known as the Soo) Railway has gone into the tree planting with a vim. Beginning in 1914, they have already planted trees along 250 miles of their right of way and they have 70 miles more ready to plant in the spring of 1918. Their plan is to prepare and plant 100 miles of way each year. So far the tree planting on the Soo Railway has been in North Dakota, but next year some tree planting will be done to protect the right of way in Wisconsin.

The tree planting is under the supervision of T. A. Hoverstad, Agricultural Commissioner for the Soo road, but formerly superintendent of Farmers Institutes in North Dakota, where he lectured to the farmers on how to protect their homes from snow and wind by tree planting. In the early nineties Mr. Hoverstad planted two experimental forest plantations in western Minnesota for the University of Minnesota, the earliest work of this kind done in this section. His long experience in forestry work has been a good preparation for the work on the railroad.

In this work the first problem that came up was getting the trees planted. One man can set about a hundred trees a day by hand, and at that the trees were not always set in the best way. Mr. Hoverstad solved this problem by inventing a tree planter. With this three men can average 8,000 trees a day with a gasoline consumption of 8 to 10 gallons for the tractor. This is as many trees as 80 men could set in a day by hand. This was not the only advantage as the machine could be set to go at a certain depth and all the trees would be planted this depth. And again with the tree planter, moist soil is packed about the tree roots, while in hand planting if the soil is dry on top this dry soil will run into the hole and cover the roots.

The tree planter is made up of a subsoil plow with two upright mold boards fastened vertically and placed six inches apart. This serves as the furrow opener. It can be set to go as deep as 12 inches. The furrow opener is followed by two disks that throw the soil in and these are again followed by two press wheels that pack the soil about the tree roots. In this way the furrow is closed as soon as the tree is set into it and there is no way for the soil to dry or for dry soil to cover the tree roots. Seats are provided for two men, one on each side of the furrow opener, so that they may conveniently drop the trees into the furrow. A larger percentage of the machine-planted trees live than of those planted by hand.

The next problem was what kind of trees to plant. In this connection one must remember that these trees are to be planted to protect railroad cuts, which means

that they will be planted on hills and knolls and that these are often gravelly and usually covered with a thin soil. Willows will grow under these conditions and their nature of growth is such that they check the wind and so stop the snow. Several kinds were tried out. The laurel leaved willow has proved the hardiest and is the one that will be used most extensively in North Dakota. The willows are planted in the outer row and also in some of the rows nearest the track. The buffalo berry, caragana, buckthorn and artemesia are also used in the outside row, the aim being to have a low growing spreading tree or shrub in the outside row, one that is quite dense near the ground so that there will be no big openings for the wind to shoot through. The artemesia dies down each year but the stems hold many of the leaves and remain upright during the winter so that they make quite an effective barrier to the wind even the first year. The artemesia will grow under very severe conditions. Willows are cut back to cause them to thicken up. When cut back in the spring they send up a great many shoots that grow to a height of four to six feet by fall, making a good protection for the winter. The second row is planted to green ash or cottonwood. The third row is



The service of the trees

Showing how the snow is held by trees, and a convincing argument in favor of planting along railroads for this purpose

planted to green ash or box elder, and the remaining five rows are planted to willows. These will be cut back periodically one row at a time. Eight rows of trees are planted on the north and west sides of cuts and six rows on the south and east sides. At first but four rows were planted on the north and west sides and three on the south and east sides, but this did not give enough protection, so it was decided to increase the plantings.

The fifty-foot right of way on each side of the track was found to be too narrow for effective tree planting, so now 75 feet in addition are being bought on the north and

west sides of cuts, and 50 feet additional for the south and east sides. The trees are set three to four feet apart in rows eight feet apart. The land that is in native sod is given two years' preparation and that which has been in cultivation is given one year's preparation before the trees are planted.

A nursery has been started and the trees are being raised for the railway planting. So far most of the trees have had to be bought from nurseries. Many of these old trees have been secured at from \$2.50 to \$6.50 per thousand. The trees from the railroad nursery have done the best, probably explained by the fact that it is on sandy land and under the same climatic conditions as those under which the trees are to grow.

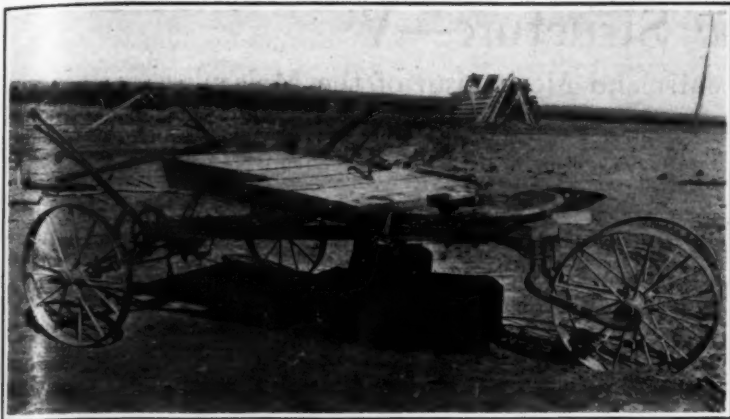
The trees are taken in refrigerator cars to the cuts where they are to be planted. In this way it has been found possible to keep the trees dormant until July. The cars are placed at the nearest railroad station and the trees hauled out to the cuts and heeled in until needed for planting.

This tree planting is encouraging many farmers along the right of way to plant trees. One of the drawbacks in successful tree planting is often that the land is not properly prepared. Any one who observes the way the trees on the right of way are planted can readily learn how to do it on adjoining farms. There is no patent on the tree planter here described, so anyone may make and use it.

While most of the tree planting has been done in North Dakota, it is also needed on the cleared land in the states originally wooded. Mr. Hoverstad is advocating buying the extra right of way before the land is cleared. It does not cost much then and the trees are already on it. After the land is cleared it becomes much more expensive and difficult to buy and then it must be planted.

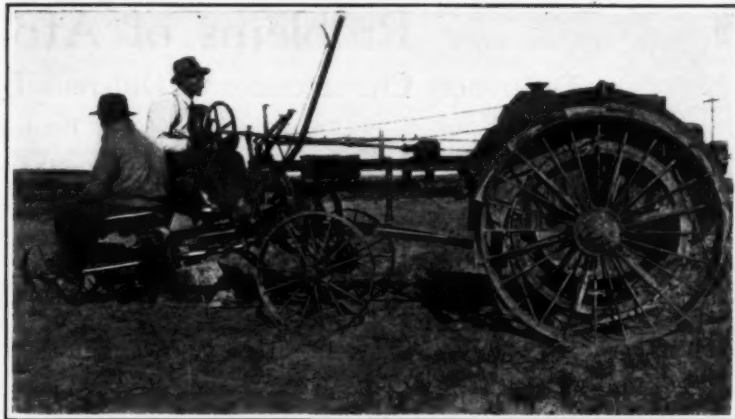
While the tree planting has not been carried on long enough to have much data on the cost of it, enough has been done so that a general idea can be formed as to how the cost of protection with board panels compares with the cost of securing protection by tree planting. It takes 640 16-foot panels to protect a mile of right of way with a single snow fence. These cost at least \$2.50 each, or a total of \$1,600. The annual depreciation is 20 per cent, or \$320. The cost of setting up and taking down is about 20 cents each, or \$128. Interest at six per cent is \$96, or a total of \$544 per mile per year. Planting eight rows of trees on one side of the track and six on the other will require 25,000 trees for a mile. At five dollars per thousand this will amount to \$125. The cost of planting will be less than \$50. The trees will occupy 15 acres and if the cost of preparation is \$15 per acre this will amount to \$225, or a total of \$400. No charge has been made for interest. There will need to be some replanting and the trees will need to be cultivated three or four years. After that the trees should not need much attention except that the willows will need to be cut back occasionally, and for some time what is cut out will be used for cuttings. It must be remembered also that the panel snow fence is not always effective so that the loss of traffic in case of snow blockade may be more than the cost of the snow fence and with it there also comes the loss of prestige. The trees when three or four years old and

\*Courtesy of the American Forestry Magazine, Washington, D. C.



A great labor saver

Showing detail of original tree planter, which may easily be constructed by anyone, as no patents have been taken on it. This is highly efficient and solves one of the important problems of tree planting on a large scale



The improved labor saver

The new tree planter, attached to a tractor. Note the furrow opener, the disk for throwing the soil in and the press wheels. It is with this outfit that three men can on an average plant 8,000 trees in an eight-hour day

planted properly will thereafter furnish effective protection.

It will be seen that the tree planter has done much toward making the tree planting a success, and such tree planting is proving an effective method of keeping railroad cuts free from snow, even in the winter with the big snows. Without the increased efficiency which these planters make possible tree planting on a large scale would have been so laborious as to be practically prohibitive. By using the tree planters large areas are covered in a short time.

#### A New British Oil Industry

BRITISH sources from which oil may be obtained by distillation are oil-shales, coal, cannel coal and torbanites, blackband ironstones, lignite, and peat. Of these the only source at present being utilized is oil-shale, which is mined and retorted only in Scotland. From this source not more than about 300,000 tons of oil fuel for the Navy can be obtained in a year's working. Of the other sources of liquid fuel, coal yields too little and is too valuable to be utilized on a large scale, lignites are not yet available for development on a sufficient scale, and peat has proved troublesome and expensive to treat owing to the difficulty of eliminating water. The remaining possible sources of oil, viz., cannel coals, torbanites, and blackband ironstones, are closely associated, and are, in many cases, easily obtainable. Most cannels contain a very large amount of "jetonized" spores, which are believed to yield on distillation waxes and resinous compounds. Torbanites, though externally similar, are essentially different from true cannels. They consist of a coal matrix in which are embedded yellow or brown globules of "kerogen." These globules vary greatly in size, shape, and number, and contain a considerable proportion of inorganic matter in close, and apparently chemical, combination with hydrocarbons of organic origin. They undergo a degeneration of which four stages have been distinguished, and the quality of oil (paraffins and olefines) yielded on distillation varies according to the stage of this degenerative process, the most degenerated types yielding quantities of heavy paraffin waxes, while the fresher material yields a greater proportion of light oils. Every gradation between a typical torbanite and a typical cannel exists. Owing to the high ash percentage in cannel coals (20 or even 30%, and seldom under 10%), large quantities of cannel have been dumped as useless for fuel purposes, together with impure coaly or canneloid bands ("Jacks, Gees, Rattlers, Batts," etc.) which could not under any pretext be sold as coal. All these deposits, however, contain kerogen globules and uncarbonized vegetable matter from which oil can be obtained on distillation. Blackband ironstones contain iron oxide or hydroxide in very intimate association with carbonaceous material which is kerogenous or only slightly carbonized, and is thus capable of yielding oil by distillation. Hitherto, blackband ironstones have been prepared for the blast furnace by being burnt in the open with a quantity of admixed coal, but it has recently been demonstrated by Hollingworth (Eng. Pat. 18,103 of 1914; this J., 1915, 1255) that the oil contents of these ores can be extracted by distillation, the nitrogen content being obtained as ammonium sulphate, and sufficient non-condensable gas produced to supply the heat required for the retorts. The residue, designated "carbousiron," consists of ironstone with intimately associated fixed carbon. It is now proposed to exploit the various deposits in this country of cannels and bastard cannels, torbanites, and blackband ironstones, by low-temperature carbonization, for the extraction of oil and all possible by-products. It is considered probable that over

the whole country the cannels and torbanitic cannels will yield from 33 to 35 galls. of crude oil per ton. The petrol obtainable will probably vary from 4 to 10, and the oil fuel from 60 to 70% of the crude oil. The average yield of ammonium sulphate may be estimated at 30 to 40 lb. per ton. The amount of solid paraffin wax recoverable will probably be less than is obtained from the Scotch oil-shales, but an advantage which cannels have over the oil-shales is that the retort residues from the poorer grades can be worked up into very fair briquettes, while those from the better grades can be utilized in producers, to give not only all the power-gas required, but the full yield of ammonium sulphate. Only large central works, situated with due regard to supplies and facilities for transport, and capable of dealing with at least 1,000 to 2,000 tons of cannel per day are projected. At least six such retorting and refining works could be established without any difficulty. The goal aimed at is some 400,000 tons of oil fuel per annum, and some 6 or 7 million gallons of petrol, besides intermediate oils.

The urgent importance at the present juncture of increasing the country's oil-fuel supplies is emphasized and the question of the relative advantages of high- and



Much depends on its proper protection

A railway cut. These must be kept from drifting full of snow, and proper tree planting has proven most effective in accomplishing this result

low-temperature carbonization of coal, etc., discussed. High-temperature treatment is necessary for the production of benzol and other aromatic compounds required for the manufacture of high explosives, but the yield of oil is low. Low-temperature carbonization on the other hand gives a maximum yield of oil, but aromatic compounds are absent. By passing steam through coal as it is being carbonized at a high temperature in vertical retorts, considerably larger yields of gas and tar are obtained, but the tar contains very little benzol and toluol.

The methods employed in the testing of cannel coals for yield of oil are described, and the nature and refining of the crude oil discussed. The results of several tests are given. Crude cannel oil has the following properties:—Sp. gr., 0.887 to 1.000; sulphur, 0.4 to 1.1%; setting point, 5° to 30° C.; water, after partial separation, 1 to 4%; calorific value of dry oil, 9.550 to 9.655 cal. per grm. (17,190 to 17,379 B.Th.U. per lb.). The following fractions are obtainable by refining:—Benzine, approximately 3%, sp. gr. about 0.740 to 0.750, with a final boiling point of 170° C. Intermediate oil, approximately 12%, sp. gr. about 0.800, flash point about 80° F.; such an oil would be an excellent light fuel for tractors, etc. Navy fuel oil, approximately 50 to 60%; sp. gr. about 0.870, flash point over 170° F., and setting

point below 25° F. Paraffin scale, variable, averaging approximately 3 to 4%. Pitch, hard or soft as required, more bituminous in character than coal-tar pitch, and at the worst, mixed with oil, would produce a works fuel. —Abstract in *Jour. Soc. Chem. Ind.* on papers by E. H. CRAIG; F. M. PERKIN; A. G. V. BERRY and A. E. DUNSTAN at the Inst. Petroleum Tech.

#### The Present Position of Metamorphism

THE present position and outlook of the study of metamorphism were discussed. For the first time it seems possible to approach the subject of metamorphism systematically from the genetic point of view. For the geologist this implies the critical study, not only of the great tracts of crystalline schists and gneisses, but equally of metamorphic aureoles, of pneumatolysis and other contact effects, and of the phenomena, mechanical and mineralogical, related to faults and overthrusts. It implies, moreover, the recognition that these are all parts of one general problem, that of the reconstruction of rocks under varying conditions of temperature and stress. This problem is complicated by the fact that perfect adjustment of chemical equilibrium cannot be assumed, either in the rocks prior to metamorphism, or during the process of metamorphism itself. The most fundamental characteristic of metamorphism was considered, namely, that recrystallization takes place in a solid environment, and so may be profoundly affected by the existence of shearing stress. Stress of this type arises from the crystal growth itself, and is called into play by external forces. The automatic adjustment of the internally created stress to neutralize that provoked from without affords the key to all structures of the nature of foliation. The mineralogical peculiarities characteristic of the crystalline schists must find their explanation in kindred considerations; for it can be shown that the chemistry of bodies under shearing stress differs in important respects from the chemistry of unstressed bodies. The result is seen in the appearance of a certain class of "stress-minerals" where the dynamic element has figured largely in metamorphism, while in the same circumstances, the formation of minerals of another class seems to have been inhibited. The conditions governing metamorphism are temperature and shearing stress, with uniform pressure as a factor of less general importance. If the orogenic forces are sufficient to maintain shearing stress everywhere at its maximum, the stress itself becomes a function of temperature, since this determines the elastic limit, and the principal conditions of metamorphism come to depend upon a single variable. This degree of simplification, however, is not to be expected universally.—DR. A. HARKER before the Bristol Geological Society.

#### Cost of Producing Calcium Carbide

THE essential requirements for the production of calcium carbide at a low figure are an abundance of cheap raw material and cheap power. In Japan conditions are particularly favorable in these respects. The cost of production prior to the war was from 3 yen to 3.50 yen per 100 lb.—yen = 2s. 0½d. at par—but it has gradually increased during the past three years in consequence of the advanced cost of raw material and labor. At present the average cost of production may be estimated at approximately 5 yen per 100 lb. Owing to the enormous demand in Japan for sulphate of ammonia, and the consequent rise in prices, one Japanese carbide factory, with a capacity of from 25,000 to 30,000 tons per annum, is now concentrating its efforts on the production of sulphate of ammonia, the ammonia being made from calcium cyanamide produced from the carbide by the fixation of nitrogen drawn from the air. —*The Engineer.*

# Problems of Atomic Structure—V\*

## Differences Characteristic of Different Elements, and Mechanism of the Molecule

By Sir J. J. Thomson

[CONTINUED FROM SCIENTIFIC AMERICAN SUPPLEMENT No. 2213, PAGE 347, JUNE 1, 1918]

IN opening his fourth lecture the speaker said that on the last occasion he had suggested that spectra might possibly originate by an electron falling from one position of equilibrium inside the atom to another. In this fall a finite amount of energy was liberated. The idea brought forward as to how this event arose was that the disturbance to which the atom was subjected altered the repulsive force represented by the term  $\frac{h^2}{r^2}$  in the law

proposed for the interaction of a positive and a negative charge. The disturbance was assumed to increase the repulsion and the electron was accordingly displaced to a new position of equilibrium further away from the nucleus. One of the hypothetical Q particles was then suddenly ejected and the electron fell in, and in doing so gave out a unit of radiation, which carried away with it a definite amount of energy, equivalent to that which the electron had gained in falling from the one place to the other. On this view, all radiation was built up of definite units, so that light itself had a molecular structure.

If this were the case, the question arose as to how these units acquired the regularity of phase actually observed in optics. It was not enough that the units of light should have the same periodicity, but there must also be a continuity of phase as they passed through transparent media. This continuity of phase was particularly notable in Michelson's experiments on long-distance interference. His results appeared, however, to be not due to any action in the system emitting the radiation, but to the action of the medium through which the radiation passed.

Newton, in his corpuscular theory of light, which bore a good deal of resemblance in its fundamentals to modern views, had suggested that his corpuscles were subject to alternating fits of easy transmission and of easy reflexion, so that it was almost a matter of chance whether one of his particles on encountering a molecule was reflected or transmitted.

It was of interest to determine how such a property as that assumed by Newton could be established in the case of the electric systems of which it appeared the atoms and molecules were constituted. What would be the effect of rapid changes of phase on these systems as the units of radiation came up? When light passed through a transparent medium, this medium was itself affected. The electrical systems comprised in it were thrown into forced vibrations, with a period (that of the light unit) differing from their natural period. If the elements of light were uniform in phase these forced vibrations settled down, and there was no loss of energy. If, however, the phase of the light was irregular, there would be a period of disturbance before this stage was attained, during which the medium would emit light of its own periodicity, and there would be a corresponding dissipation of the energy of the incident light. Light of variable phase was accordingly easily absorbed, the loss being far greater than when the phase was continuous. Units in the proper phase would, on the other hand, get through the medium without much loss of energy. The medium, therefore, exerted a sorting action on the units which reached it, making them come into step. In this way uniformity of phase would be impressed on the light units even should these be emitted almost at haphazard.

The view above advanced as to the origin of radiation implied that an electron might have more than one possible position of equilibrium inside the atom. If in the case of hydrogen, for instance, there were two such positions, we should have two different kinds of hydrogen atom. With the electron far out, the refractive index would be greater than when the electron was closer to the nucleus, and it would, moreover, be easier to extract an electron from the outer position of equilibrium than from the inner one, and there would thus be two ionizing potentials. There was, however, no evidence in normal hydrogen of the existence of these two kinds of hydrogen atom. If, however, the matter were considered more closely, it would be noted that the special state corresponding to the far-out position of the electron was itself supposed to be produced by the same cause as that which produced the light; and this special state was therefore not a normal one. It was only when hydrogen was in a state ready to emit light that the abnormal atom was to be found; and it was noteworthy that when hydrogen was

self-luminous it had some special properties, certain absorption lines being then apparent which could not be detected in the normal condition of the gas. The fact that we could not separate normal hydrogen into two kinds did not accordingly constitute an argument against the views advanced.

The shortest wave-length in a spectrum was that associated with the greatest energy. The emission of light of this wave-length corresponded to the fall of an electron to that point of equilibrium which lay nearest the nucleus. There was thus on this view a limit to the frequency of the light emitted by H and, in fact, such a limit was indicated by Balmer's series, which converged to a value of  $\lambda = 3646 \text{ \AA}$ . This was the limit of the most conspicuous series in the hydrogen spectrum.

Schumann had, however, discovered a new kind of radiation of much smaller wave-length than ordinary, and this discovery had been developed by Lyman, who had found that hydrogen was capable of emitting radiations with a wave-length of between 800  $\text{\AA}$  and 900  $\text{\AA}$ , or only one-quarter of the limit of Balmer's series. Hence, if the spectra originated as suggested, the Balmer

Fig. 1.

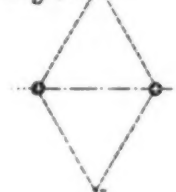


Fig. 2.

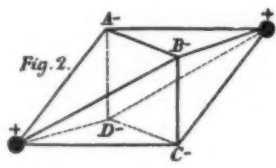
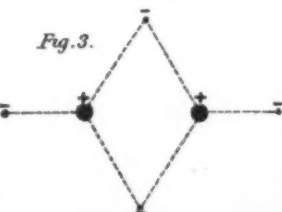


Fig. 3.



series was not produced by the fall of the electron to the absolute minimum of distance from the nucleus; and it must be possible for this electron to get nearer to the center than it ever did when producing the lines of Balmer's series.

In the foregoing it had been assumed that was light produced by the sudden transfer of an electron from one position of equilibrium inside an atom to another, and it was of interest to inquire whether a change of the same sort might not occur in the case of compound molecules.

In a previous lecture<sup>1</sup> he had suggested that the hydrogen molecule consisted of two positive nuclei bound together in stable equilibrium by electrons as indicated in Fig. 1. Suppose now that two atoms, each containing two electrons, formed a molecule, then there were a number of possible alternative positions for the two additional electrons. The arrangement might be for example, as indicated in Fig. 2, with the electrons at the points A, B, C, D. In another alternative, probably more stable, the square A, B, C, D would be replaced by a triangle, one electron being at the center of this and the other three at the corners. Still another alternative was represented in Fig. 3, where the additional electrons lay outside of the positive charges instead of between them. With all the electrons between the positive nuclei we had a representation of what chemists called double bonding, whilst with two of the electrons outside the bonding was single. If the electrons swung round from the system of bonding represented in Fig. 3 to that in Fig. 2 there would be a definite change of energy. From a mechanical point of view, the effect was much the same as when an electron changed its position inside an atom, and we might expect an emission of light. Hence, if we subjected a molecule to any process which altered the bonds between the atoms, we might get radiation, and

this radiation would not be accompanied by the actual ejection of an electron. There would thus be no ionization and no conductivity. Light might thus be produced by merely altering the arrangement of the chemical bonds.

It was possible also to account in this way for certain remarkable changes in the color of certain compounds which occurred without any detectable change in the chemical composition. Fluorescence also might be due to the existence of two isomeric compounds differing only in the linkages by which the atoms were united. The fluorescent light would be emitted in the change from one system to the other. There were, the lecturer said, many cases in which great changes of color were brought about by very slight changes in the physical conditions. Thus a paper saturated with a mixture of the iodides of mercury and copper was brilliant red when cold, but became dark when slightly warmed and recovered its original color on cooling. This change was unaccompanied by any known change in the chemical composition. Similarly iodide of mercury changed its color from bright red to yellow when heated, recovering its original color on cooling. These changes were, he suggested, due to changes in the bondage by which the constituent colors were connected together.

He thought that changes in the bondage between the atoms of a compound might in many cases be responsible for the emission of light. Moreover, ionization might also arise by the ejection of an electron from the group that helped to bind together the various atoms of a compound. If we considered the arrangement of these bonds, it was evident that the linkage between the atoms might be very similar in different molecules. Here, possibly, was to be found the explanation of a rather puzzling phenomenon in the matter of ionizing potentials. The work required to extract an electron from different molecules had been the subject of many careful experiments. The results came out at about the figure that would be expected in the case of hydrogen and helium, yet considering the gases as a whole it was evident that there was a tendency for these ionizing potentials to be much the same for all, the average value being somewhere in the neighborhood of 10 volts. There was thus not much apparent connection between this ionizing potential and the chemical properties of a gas, as is clearly shown in Table I.

TABLE I.—Ionizing Potentials for Different Gases.

Gas.	Hughes.	Franck and Hertz.	Gas.	Hughes.
He	—	20.5	Hg	10.2
Ne	—	16	HCl	9.5
Ar	—	12	CO	7.2
H <sub>2</sub>	10.2	11	CO <sub>2</sub>	10.0
O <sub>2</sub>	9.2	9	NO	9.3
N <sub>2</sub>	7.7	7.5	CH <sub>4</sub>	9.5
S	8.3 ?	—	C <sub>2</sub> H <sub>2</sub>	10.0
Cl <sub>2</sub>	8.2	—	C <sub>2</sub> H <sub>4</sub>	9.9
Br <sub>2</sub>	10.0	—	C <sub>2</sub> H <sub>6</sub>	9.9

It was remarkable that the ionizing potential for nitrogen was the lowest of any, with the possible exception of CO. This approximate constancy of the ionizing potential might be attributed to mechanical impurities mixed with the gas. For example, if hydrogen were present and if it ionized more easily than other gases, the apparent ionizing potential would be that of the hydrogen admixture. It was not easy to see, however, how the figures recorded for carbon monoxide could be accounted for on this line, since the ionizing potential of this gas was less than that of either of its constituents and much less than that of hydrogen. Again it was noteworthy that such intensely electro-negative elements as oxygen, sulphur and chlorine had smaller ionizing potentials than hydrogen. It would be natural to expect that as electro-negative elements had a strong affinity for negative electricity it should be more difficult to extract an electron from them than from hydrogen. These considerations led the speaker to think that the ionization of the gases in question was effected by extracting an electron from the bonds connecting the two atoms of the molecule rather than from the atom itself.

It was of interest to note that the linkage between atoms could be altered without detaching an electron. When hydrogen and chlorine combined under the action of light to form HCl the combining gases showed no trace of conductivity. The speaker had tested this with the most delicate apparatus. It was thus evident that to form HCl it was not necessary to pick either the chlorine or the hydrogen molecule to pieces by taking out an electron, and then to put the pieces together again to form

\*A paper presented at the annual meeting of the Society of Automotive Engineers, and published in the Journal of the Society.

<sup>1</sup>See Sci. Am. Sup., No. 2213, p. 346.

the compound, restoring the electrons removed in the process. The two elements combined, in fact, without any detachment of an electron from the positive nuclei.

There was, the speaker continued, a peculiar relation of a general character between the combining weights and the chemical properties of the elements. As he had already pointed out, if we considered the atom as consisting of a positive nucleus surrounded by electrons, the maximum number of electrons in a shell was eight. There were reasons for supposing that the number of electrons in the atom was somewhere about one-half the atomic weight. That was to say, it was equal to the atomic number, which was defined as the position of the element when arranged in the order of its atomic weight. Thus, the atomic number of hydrogen was 1, that of helium 2, of lithium 3, and so on. The ordinary view was, as stated, that the number of electrons in the atom was equal to this atomic number, but the speaker would suggest that it was not strictly correct. According to another way of looking at the matter, the number of electrons present was not quite equal to the atomic number, and this view fitted in much more closely with the actual properties of the elements than did the ordinary view.

The evidence as to the number of electrons in the atom was all derived from a study of the scattering produced by these electrons when exposed to the action of Röntgen rays. These experiments led, as stated, to the conclusion that the number of electrons in the atom was about one half the atomic weight. This conclusion should not, however, be pressed too far, and the experimental figures might well be subject to a correction of 10 per cent or 20 per cent, due to the fact that in interpreting the results observed, the help which one electron might give to another, if the wave-length used were too long, had been neglected. It had been assumed that the effects measured were proportional to the number of electrons present, but if the wave-lengths were long compared with the distance between the electrons the effect would be proportional to the square of the number of these electrons. He did not suggest that the wave-lengths used were as long as that, but there was also no reason to suppose that the wave-length was so short that the results were simply proportional to the number of electrons present. In fact, experiments showed definitely that the energy scattered in the direction of the incident radiation was greater than in the opposite direction, which made it certain that the particles responsible for the scattering were not acting entirely independently, but that interference was taking place.

Hence a certain adjustment in the number of electrons as deduced from experiments on scattering was permissible. The awkwardness of the usual view was well shown by lithium. This element had 3 as its atomic number. On the ordinary view there would thus be three electrons in the outer ring. These electrons were, however, associated with the valency of the atom, which was considered to be equal to the number of these electrons in the outer shell. Lithium was, however, monovalent and ought, therefore, to have one and not three free electrons. Similarly, carbon had the atomic number 6 and should, on the view criticized, have accordingly six free electrons. All the evidence we had indicated, however, that the carbon atom was tetrahedral with one electron at each corner, or four free electrons in all. In both cases, taking the electrons as equal to the atomic number gave two electrons too many in the outer shell, and this rule held throughout.

The view he proposed to substitute for that hitherto in vogue was connected with a very conspicuous property of the atomic weights, which was indicated in the annexed table:

Element.	Remainder.	Element.	Remainder.	Element.	Remainder.	Element.	Remainder.
H	1	O	0	Na	3	S	0
He	0	N	0	Mg	0	Cl	3
Li	3	C	0	Al	3	K	0
Be	1	P	0	S	0	Ca	0
B	3			P	3		

This comprised a list of all the elements up to calcium and the figures given showed the remainder left after dividing the atomic weight by four. With but two exceptions, Be and N, this remainder was either zero or three. If no law were involved the residue was equally likely to be 0, 1, 2, or 3.

He thought that the explanation of this striking peculiarity was bound up with the constitution of the central nucleus of the atom. This nucleus was considered to be made up of an aggregation of unit-charges of positive electricity, each equal to the hydrogen nucleus. If we took this view, something was required

to drag the components of the nucleus together. If the positive charges repelled each other to the bitter end, such a nucleus could not exist, and he had accordingly suggested that the law of force was such that at sufficiently small distances the repulsion changed to an attraction. He thought a study of this positive nucleus would repay attention. For the present he would only point out that a certain constitution of these nuclei would involve the property that the number of electrons was two less than the atomic number and would thus fit in with the chemical valency. He could, however, give no reason why the nucleus should have the peculiar structure he was about to describe.

He would suppose that the atoms comprised in their nuclei elements like the nucleus of helium, which contained four units of positive electricity. Embedded in this nucleus he would assume that there were two electrons. As another constituent element of atomic nuclei he would suppose another group of four unit charges, but the charges in this case were units of negative electricity. Now from the table given in Lecture II., four electrons could not be held together in stable equilibrium by two units of positive electricity. There must be three or four such units. He assumed that the nucleus of all the stable elements was built up of elements like the helium nucleus, together with other tetrahedral elements in which the positive charges at the corners were replaced by negative charges, these being held in position by a sufficient number of positive charges, viz., either three or four.

With this constitution possible atomic weights would be  $4p +$  the positive charges in the negative units. All chemical elements would thus have atomic weights represented by  $4p+0$ ;  $4p+3$ ; or  $4p+4$ ; so that dividing the atomic weight by four would leave either zero or three as the remainder. This result was in accord with the table given above and the valency also came out correctly. Thus lithium, with an atomic weight of 7 would have at its center one helium nucleus giving four positive charges and two electrons embedded therein, and one "inverse" nucleus with four negative charges and three positive charges. One additional electron would thus be required to render the atom neutral, and this would lie in the outer ring, giving a mono-valent atom. Similarly, carbon might be conceived as having its center built up of two helium units, together with one of the inverse negative units. The two helium units would imply the existence in the center of the atom of eight positive charges and four negative ones. The one negative unit would bring with it to the center four negative charges and four positive ones. The atomic weight being equal to the number of positive charges would then be 12. The total number of negative charges bound up at the center would be eight, leaving four for the outer shell, and the atom built up in this way would be tetravalent.

Atoms constituted on these lines would contain two electrons less than their atomic numbers, and their valency would be in accord with experience.

[TO BE CONTINUED]

### Magnesite

The value of this refractory material depends not only on its resistance to the corrosive action of heat and metallic slags, but also on the permanence of the forms in which it is put into the furnace. This permanence is due to a natural bonding which tends to make the loose crushed material cling together under furnace heat and thus makes brick forms molded from it more durable. Bricks and granular furnace bottoms made of magnesite that lacks this bond break, and the magnesite floats off on the fluid molten metal and is lost in the slag. Thus, though magnesite that contains a small percentage of iron may be somewhat less resistant to extreme heat than a purer form, the slight fusibility given to the material by the iron tends to hold it in place. For this reason, in part, a type of magnesite so far found only in Austria and Hungary has been the principal source of the refractory magnesia used in this country. The purer magnesite from Greece, California, and elsewhere is used in making plaster or cement or material for other relatively minor uses.

Magnesite is reduced to magnesia either in "dead-burned" or sintered form, or in what is known as "caustic calcined" form. Dead-burned or sintered magnesite has been so strongly heated that essentially all its carbon dioxide and moisture have been driven off and most of the shrinkage taken up. In this condition it is chemically very inert—that is, it is not subject to attack or disintegration even under extreme heat. The caustic form is not so thoroughly calcined; it still retains one or two per cent of carbon dioxide and is thus a product more like

ordinary caustic lime in its properties, although not so active chemically. Caustic magnesite "slacks" slowly when exposed to the air, recombining with moisture and carbon dioxide. Combined with calcium chloride it forms a distinctive cement known as Sorel or oxychloride cement, which is much favored by builders for floors and other places where special finish is desired. This is probably the most important use to which pure caustic calcined magnesite is put, although it is used also for making liquors in which wood pulp is digested to make paper, as well as for other purposes.

It is difficult to determine just how much magnesite was formerly used in the various industries, as for instance in making steel, for copper furnaces, for laying cement flooring, in making paper, and for other purposes. The greatest part of the magnesite used goes for the manufacture of steel and copper. It has been estimated that about six pounds of calcined magnesite was formerly used for each ton of steel made by the basic open-hearth process. Although the production of open-hearth steel has steadily increased during recent years, the consumption of magnesite has fallen off to such an extent that it is quite clear that the ratio of magnesite used per ton of steel produced is very much less than it formerly was. The magnesite-cement flooring trade in the United States is an important industry, though the high price of caustic calcined magnesite and the uncertain quality of the material supplied from some sources have undoubtedly greatly restricted its use in cements. The making from magnesite of digestion liquors for the paper industry is confined chiefly to companies on the Pacific coast, which have depended on the supply from California deposits. Now that magnesite is so much needed in metallurgical industries and that its value has increased so much, it is possible that some of the magnesite used in making paper may be replaced by dolomite or other cheaper materials, especially in view of the availability of dolomite in place of magnesite at a considerably lower cost.—"Mineral Resources of the United States," Part II.

### The Hadfield Prize

#### For a New Method of Determining the Hardness of Metals

SIR ROBERT A. HADFIELD, D.Sc., D.Met., F.R.S., Vice-President, has placed in the hands of the Institution of Mechanical Engineers the sum of £200, which with any income therefrom may be awarded at the discretion of the Council of the Institution as a prize or as prizes, for the description of a new and accurate method of determining the hardness of metals, especially of metals of a high degree of hardness.

The ordinary tests of hardness, such as are described in the Report of the Hardness Tests Research Committee (Proceedings of the Institution of Mechanical Engineers, 1916, pages 677 to 778), which should be consulted by competitors, fail to some extent when the hardness of the material exceeds about 600 to 800 Brinell. What is desired is the description of a research for or an investigation of some method of accurately determining hardness, suitable for application in metallurgical work in cases in which present methods partially fail.

The award or awards will be made by the Council of the Institution of Mechanical Engineers, whose decision will be in all cases final.

The Council will consider annually all communications received, and may then award a prize or prizes. But in January, 1922, the offer of prizes will be withdrawn, and any unexpended balance of the prize fund will be diverted to any other purposes to be determined at the discretion of the Council.

The Council may award the whole or any part of the sum available at any time if a communication is received which, in their opinion, is of sufficient originality and importance and satisfies the object aimed at; or they may from time to time award portions of the fund, not exceeding in all £75 in any one year, for communications which do not completely solve the problem, but which appear to advance the knowledge of methods of testing hardness. The Institution will probably be willing and reserve the right to publish in its journal any communications for which a prize is awarded.

A communication should be accompanied by scale drawings of any new apparatus described, or by a model or an example of the apparatus itself. If the communication describes a new invention, likely to be of commercial value, it is desirable that provisional protection should have been obtained before it is submitted for consideration.

Communications should be addressed to The Secretary, The Institution of Mechanical Engineers, 11 Great George Street, Westminster London S.W., 1, and marked "Method of Determining Hardness," and should reach him at least one month before the 1st January in any year.



New Zealand gum diggers at home



Transporting the gum to Auckland

## Gathering Kauri Gum in New Zealand

### A Little Known but Important Industry

By Harold J. Shepstone

A COMPARATIVELY little known but nevertheless important industry is that of gathering Kauri gum. It is only found in New Zealand, in the northern Island. Here it may be added that Kauri gum is a resin and an article of great commercial value. In fact, the world could hardly get along without it. It is from the gums or resins that we make varnish, turpentine, metal polish, paint, and such-like products which are used today for preserving almost everything that exists. We paint our battleships, steel bridges, and buildings as much to preserve them as to give them a new and smart appearance. For the same reasons we varnish our furniture and the wood work of our dwellings and polish our boots and brass-work. These resins or gums are also employed in the manufacture of a host of articles. They are largely used in the making of linoleum, and the vast rubber industries could not get along without them.

The most prized and sought after of these resins, on account of its high qualities, is Kauri gum, which curiously enough, has only been found in New Zealand. It is the solidified turpentine of the Kauri, a wonderful tree which flourishes in New Zealand. The gum is found in the earth and dug up like peat, and also on the forks of living trees. That taken from the ground is the fossilized remains of what were, in the ages long ago, forests of splendid Kauri trees. The extent of the industry may be gauged when it is stated that between eight thousand and nine thousand tons of this gum is exported annually from New Zealand. The total for 1913 was 8,780 tons of which 3,995 tons was purchased by the United States. Indeed the latter country has always taken the bigger share of this New Zealand commodity. In 1914 we took 4,531 tons, more than one-half of the total obtained.

There is no record as to who was the first to discover the valuable deposits of gum that exist today in the northern parts of New Zealand. The Maoris knew of their existence long before the coming of the British, and it is thought that they pointed them out to the early settlers. In any case, New Zealand has exported this article since 1847, soon after Britain took possession of the islands.

For many years New Zealand was content with \$25 a ton for the gum. Then it jumped to \$50, and gradually rose in value until it reached its present price of \$250 to \$300 a ton, according to quality, some twenty-five years ago. The increased price was not due to any scarcity of the article, but rather to the discovery by the world's leading chemists of its wonderful properties and the valuable uses to which it could be put.

The gum-bearing districts cover an area of just over 800,000 acres. At first gum-digging was exceedingly profitable on account of the rich finds that awarded the seeker and the ease with which it was obtained. It was found on the surface, or barely embedded in the soil. When this surface "crop" had been removed, the digger had to search for it below the soil.

For this purpose he uses a spear ten or twelve feet in length. With this weapon he pokes the ground in search of the decomposed stumps to which the gum is frequently attached, bringing it to the surface by means of a hook. The gum is found in lumps, varying in size

from that of a walnut to a man's head. Occasionally pieces are found weighing a hundred pounds and more.

For a number of years it was the belief of many that as soon as the surface area had been worked over, the supply would be exhausted. Excavations made in the fields, however, revealed the fact that there existed, at least in many places, two, three and sometimes four layers of gum, indicating the existence of two or three Kauri forests, which on disappearing—or, as it is probable, on being destroyed by fire in ages past—left in succession their quota or layer of gum in the ground.

Thus, many fields which were thought to have been worked out proved to contain numerous valuable deposits of gum. Some of these tracts had been pur-



A typical gum digger coming home after the day's work

chased for a mere song to be converted into grazing land. Thus the new owners who had picked up the fields for a few dollars per acre suddenly found themselves in possession of rich gum land and made fortunes out of the discovery.

Today the more practical method of prospecting for the prized article is to dig the earth over to a depth of six to ten feet. As the soil is removed it is sifted and the gum secured. The methods employed by the more up-to-date diggers are not unlike alluvial gold-digging, and the reward is in much the same ratio. A spade of earth may be worth nothing, or it may contain several dollars' worth of the precious article. But over a given area a digger can rely on securing a certain return, though that return may be in the last yard or two of earth treated.

In some places gum has been discovered at a depth of twenty feet and more, but, generally speaking, few beds have been worked to a greater depth than twelve feet. Hence, it is thought that New Zealand's present supply of Kauri gum can be maintained for generations to come. Probably, too, other deposits will be found in the future.

After the gum has been recovered from the earth, it is first scraped with a blunt knife. It varies considerably in color. The best specimens are almost transparent, like amber, and these fetch high prices from collectors. A piece no larger than a thimble is worth several dollars. Occasionally specimens are found with leaves, seeds, and small insects enclosed within them.

For the most part the gum-fields are covered with fern and tea-tree and rough scrub. In the old days—when the Government regulations regarding these lands were not so rigid as they are now—the diggers, when desirous of clearing a block, used to set light to the dry ferns as the quickest and most expeditious way of clearing the upper surface of the soil. But these conflagrations often spread beyond the area intended, with the result that homesteads were burnt and some settlers had narrow escapes from a painful death besides losing valuable property. Today the fields and the industry are controlled by the Government.

The gum is found in swampy ground as well as on the drier lands. Swamp land is only worked over in the height of the summer. The gum obtained from here is of a dark color, some of it being almost black. In quality it is much inferior to the lighter shade and fetches the lowest prices. But swamp-digging generally pays well, because of the bigger yield in material.

The winning of the gum from the earth gives employment to thousands of men. Sometimes as many as seven thousand will be found working over the various fields, while at others there may be but two or three thousand. This is because prospecting for gum is resorted to when other work is slack. For instance, the new settler in the gum districts will take over the gum-digging in the "off season" and so add to his income. Also the Maoris take to gum hunting when their crops fail or when their stock of provisions gets exhausted.

Some diggers work entirely on their own account, flitting from field to field, over the Crown lands, after securing the necessary permission or license from the Government, receiving payment, of course, according to their finds. Others, again, work for recognized companies, who control large tracts and dig the earth completely over to a depth of ten or twelve feet. As a rule, an energetic digger can make from \$25 to \$30 a week in the gumfields.

It has been said of New Zealand's gum fields that they have been the means of solving her unemployed problems. To a certain extent this is true, for they are open to all British subjects who care to dig there, at a nominal license of \$1.25. On arrival at the field he has elected to try his luck in, the digger's first step is to select his storekeeper and interview him. On some fields there may only be one store, on others several; but to the digger the storekeeper is everything. It is the storekeeper that gives him his start, and should he ever get into a tight



Maori gum diggers. Note the spear used to sound for gum



Government gum diggers working a face systematically

corner through illness or any other cause, it is the storekeeper who extricates him from it. The start comprises, in most cases, the necessary materials for building a shanty and a month's provisions. At the end of the month the digger sells his gum to the storekeeper, and if the result proves him a digger capable of earning his living at this work he becomes a regular customer and is allowed credit in moderation.

The credit system of the gum fields is practically a necessity, as in most cases the digger arrives with an empty purse. The storekeeper thus necessarily runs a big risk of the digger leaving him with the balance on the wrong side of the books. This often happens, and naturally the honest and capable diggers have to meet the deficiency by paying a higher price for their provisions and receiving a lower price for their gum than would otherwise be the case. The storekeeper is accordingly a much maligned person, though it can be said of the majority that they only manage to pay their way. If a storekeeper is too harsh or unreasonable his creditor can always appeal to the Government.

As soon as the digger has obtained the necessary credit to enable him to start—a month's provisions, a dozen new corn sacks, twine, needles, and nails, he is conveyed by the storekeeper by bullock-drag, pack-horse, or horse-trolley, as the case may be, to the particular spot on the gum field where he has elected to camp. His first step is to erect his hut. This, as a rule, is a very primitive affair, consisting of a "manuka" frame with roof and sides covered with sewn corn-sacks, and an earthen chimney. The cooking is all done by camp oven and billy, the ubiquitous kerosene tin playing a large part in the kitchen department. The furniture consists of a sack-bunk and packing cases adapted for various purposes.

This simple dwelling can easily be erected in a day. It has the advantage of being light for transport purposes, and is very easy to pull down and reërect. This is necessary, as the digger does not as a rule, stay long in one camp, but shifts to different portions of the field as the spirit moves him.

Many of the men who regularly follow this vocation and are married have quite comfortable cottages of timber and iron, surrounded by neat gardens. Many of them own stock and do a certain amount of farming.

The actual fare of the digger is plain, but substantial. He is always, of course, his own cook. On most fields he is able to purchase fresh meat delivered to his camp at least once a week. On a few fields he can purchase bread, but as a rule he must be his own baker; and "tinned tiger," as he terms his tinned food, figures largely in the menu. As a rule, the average cost of living for a British digger, inclusive of tobacco, an important item, and small sums for clothing and tools, runs into about \$5 a week, so that if the prospector is capable of steady work and is in a good field, there is no reason why he should not make a fair return.

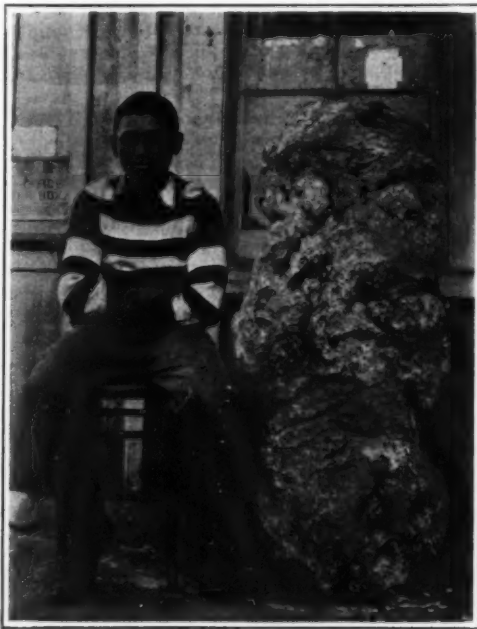
The various methods resorted to for obtaining the coveted article have already been referred to. No doubt many men are attracted to the fields by the element of chance. The digger constantly lives in the hope that he will strike a "patch," which is no unusual thing, though every year the finding of a patch becomes more improbable. A patch may consist of any quantity of gum, and many instances are on record of a man, or a couple of men, earning over \$500 in a single week. The chances are, however, that a man will dig gum all his life without uncovering a patch of this sort, and it is the steady, hard-working, plodding individual who labors several hours daily that has the bigger check to draw at the end of the month.

Naturally, year by year the gum becomes scarcer as it

is constantly taken out of the ground. The supply is kept up, however, by new fields being opened which were not seriously considered before, perhaps on account of the quality of the gum hitherto found in them or the fact that it was believed to exist only in very small quantities.

Then, whereas a decade ago the poorer qualities were unmarketable, there is a ready sale for them today. In fact, to obtain these poorer classes of the article the very ground out of which the gum has been taken is sieved in the water-holes to secure the precious dust and nuts that are left. This is today a common form of gum hunting among the natives. They can, where plenty of water is available, in a good locality, secure at least two full sacks a day of the riddlings, getting about \$2.50 a sack for them. It is, however, very wet, unhealthy work, the digger being up to his thighs in water all day long.

Another method of winning the prized commodity is by draining swamps. A number of men will secure a marshy tract of land, drain it, and then take out the gum. Good results are often obtained in this way. As a rule,



A large nugget of Kauri gum weighing about 225 pounds

however, the New Zealander and the Britisher prefer to work on their own. In fact many of them are attracted to the fields because they are their own boss—they can work when and how long they please. To some men this is a good thing and an incentive to work; in others it proves the reverse, as they idle half their time away.

Every evening the gum won during the day must be scraped and put away. This means at least two hours' steady work daily to a man who obtains only an average amount of gum. To sell the article is a much simpler matter than digging it, and on the appointed day, as a rule monthly, the gum is shot out and the buyer comes round and examines it. It is then sacked and weighed, and after the usual argument as to price—in the opinion of the digger, gum is always up, and in the opinion of the buyer always down—the gum is sold. The amount outstanding from the store account is deducted and the balance handed over to the digger.

A stranger visiting the fields is often surprised to find so many foreigners among the diggers. This has led many to regard the fields as the Land of the Lost. This, however, is certainly not the case. So far as scenery is concerned the gum fields are decidedly dreary localities, but the majority of the prospectors, with the exception of a few deadbeats who take to the fields as a refuge, live a happy enough life, are good sportsmen, and generous. Among them you will find old soldiers, old sailors, bank clerks, lawyers, remittance men, and men of every nationality.

Curiously enough, among the foreign element, Austrians predominate, chiefly emigrants from Dalmatia. When war broke out there were a thousand of them working on the gum fields. They were informed that if they behaved themselves and remained loyal to the country they could continue their work. What seemed to depress them most was the fact that they could no longer send remittances to relatives and friends in Austria. Foreigners can only dig over private fields, and have to pay a licence of \$10. Should they become naturalized British subjects the license is only \$1.25 and all the fields are then open to them.

At first all the diggers were greatly concerned about the war. The closing of so many European countries and the general shortage of boats made them nervous as to whether it would not bring down the price of the commodity. But the Government stepped in and rightly decided to take the gum at the then ruling prices and store it in Auckland. A few of the more wealthy diggers are storing their gum on the fields in the hope of realizing a higher price after the war. Kauri gum, a commodity which the world in general would find it difficult to do without, should certainly be plentiful when things again become normal, and for many years to come the winning of this strange substance from the earth will continue to be one of New Zealand's leading industries.

#### Vitamines in War-time Diets

This was the topic of a recent lecture given by Miss E. M. Hume of the Lister Institute at University College, London. Among the deficiency diseases to be feared at the present time, rickets, beri-beri and scurvy are the more important. Fresh milk, butter and codliver oil are the best available preventives of rickets, and for children, butter should be chosen in preference to margarine. Beri-beri develops when the diet consists too largely of over-milled cereals and super-heated (tinned) foodstuffs. It occurred in Gallipoli and Mesopotamia, among white troops, on a diet consisting mainly of white bread and meat, partly tinned and partly fresh; Indian troops were protected from it, because their ration included wholemeal and peas, beans or lentils. Scurvy became rife among Indian troops in Mesopotamia when fresh vegetables failed; white troops were protected by their large daily rations of fresh meat which most Indian soldiers refuse to eat. Indian troops could be protected against scurvy by the germination of the lentils or peas served out in their ration, for after 24 hours' germination such lentils are as valuable as fresh vegetables. Dried vegetables have always been found useless against scurvy, and commercial lime-juice seems to be equally impotent. A few cases of scurvy developed in the spring of 1917, in the northern towns of England, at the time of the potato shortage, but they disappeared when the new crop came on the market. When cow's milk has been heated or dried, or if starchy foods have been added, its anti-scorbutic property is diminished and orange juice should be given with it to infants. Failing orange juice, a larger dose of raw swede juice is an efficient substitute.—*Jour. Soc. of Chem. Ind.*

### Long Range Artillery Calculation\*

By Sir George Greenhill

RECENT events have directed attention forcibly to the importance of long range calculations in modern warfare, in opposition to the old-fashioned traditions of intellectual timidity and rule of thumb; and it is little consolation to the citizens who suffer under a distant bombardment to be assured it is of no military value.

Formerly the gunnery problem was presented to the student of theoretical artillery from a point of view the reverse of what is important today; from the data of given ballistic power of the projectile, and given muzzle velocity and angle of projection, he tried to calculate the range over a horizontal plane, the time of flight, angle of descent, striking velocity, and further, some subsidiary effects, such as drift from the vertical plane of fire; also the probability of hitting a target of given size.

In the field these details would be presented in a different order. The range would be given and then the requisite elevation looked out from a range table of the gun required to obtain this range, the ballistic power of the gun being given.

The old gunner preferred to make his own range table, based entirely on his practice, heedless of theory; but this method became too expensive with modern weapons, and ink was made to serve instead of gunpowder to a greater and greater extent in drawing up a range table, as more economical and equally trustworthy. But in our recent experience these conditions are reversed completely, and the former order of calculation requires to be turned round and inside out, and the problem considered in the reverse of the historical order. Suddenly we find one day the enemy shell begin dropping on us from the sky, and we cannot imagine whence they come, and calculation is our only guide.

Fragments pieced together tell us the caliber of the gun and weight of the shell. The slant of the hole made on impact in the ground gives the angle of descent, and the depth of the hole and other destructive effects are measures of the striking velocity. Working backward from these estimates, however rough, of the ballistic power of the shot, the striking velocity, and the angle of descent, and with the resistance of the air reversed in direction into an assistance, the usual calculations of long-range fire will determine ultimately the initial velocity, angle of projection, time of flight, and range of the shell, and then the gun is located within narrow limits, on the direction of the line of fire, corrected for drift and rotation of the earth. Because in a time of flight estimated as so great as  $2\frac{1}{2}$  minutes, the influence of the earth's rotation is quite appreciable. It is calculated on the theory that the vertical plane of the trajectory remains fixed in space, as in the Foucault pendulum, and keeps pointing to a star near the horizon, rising or setting, while the horizontal plane on the earth is turning round at a horary rate of  $15^\circ$  at the pole, but  $15' \sin$  lat. elsewhere; that is  $15 \sin$  lat. per minute of time (sidereal), or  $0.25 \sin$  lat. per second. Thus in 2.5 minutes, and in our latitude, the horizontal plane of the earth will have revolved about half a degree past the plane of the trajectory, and a deflection of  $30'$  must be given on the sight, against the sun, widdershins. In 75 miles, or 120 km., this would account for about 1 km. of deflection on Paris. The drift, too, of the shell fired from a right-handed rifling would require to be corrected by an additional deflection of considerable, but unknown amount, in the absence of an exact theory of drift, and determinable only by experiment. The total deflection reversed would be applied to the apparent line of fire into Paris, in locating the gun.

Acting on these preliminary instructions, our airmen will soon locate the enemy gun with accuracy, and endeavor to bomb it out, the target being too small to reach with distant gunfire. Meanwhile our artillerymen must set to work to make up lost ground, and strive to produce a rival weapon of equal power, capable of imparting the requisite muzzle velocity to the shell of given weight; and here all the resources of calculation of internal ballistics and the theory of gun construction are required, before the gun can be designed and then put in hand in the workshop, everyone working hard against time. Any method of scientific procedure, however rough and ready, will be useful in saving time, and acting as a check on more refined calculation, and so we proceed to describe some such methods, given already in the writer's "Notes on Dynamics," for the use of the officers of the Advanced Class of the Artillery College, 1908, §78, page 99.

As stated there, "The Parabolic theory of Galileo, although ignoring air resistance, is able to assign limits between which the real trajectory will lie in a resisting medium. The range  $X$  is supposed given, and the angle of departure and descent,  $\alpha$  and  $\omega$ ; and then the real trajectory must lie between the two parabolas on the range  $X$ , with angles of departure  $\alpha$  and  $\omega$ , and equal angles of descent. The height  $h$  of the real trajectory lies

between the height of the parabolas, so that  $\frac{1}{4} X \tan \alpha < h < \frac{1}{4} X \tan \omega$ . The time of flight  $T$  is intermediate to the parabolic times of flight,

$$\sqrt{\frac{2X}{g} \tan \alpha} < T < \sqrt{\frac{2X}{g} \tan \omega}.$$

And if  $V$ ,  $v$ , denote the muzzle and striking velocity in the real trajectory,

$$V > \sqrt{(gX \csc 2\alpha)} > \sqrt{(gX \csc 2\omega)} > v, \text{ ft./sec.}$$

The least parabolic velocity to give the range is  $\sqrt{(gX)}$ , and then  $\alpha = \omega = 45^\circ$ ; and with a range of 75 miles,  $X = 396,000$  feet, and  $\sqrt{(gX)} = 3,200$  feet per second, a velocity attained by Andrew Noble in his experimental 6-inch 100-caliber gun, using a charge of 27.5 pounds of cordite, with a shot of unspecified weight, but probably 100 pounds.

The height attained would be one-quarter the range, say, 19 miles, and the time of flight 157 seconds, a little over  $2\frac{1}{2}$  minutes.

The actual time of flight has not yet been recorded, but we may assume for our argument that it was 2.5 minutes, making the average velocity over the ground for the 75 miles equal to 30 miles a minute, 1,800 miles an hour, 2,640 feet per second. Suppose, then, we imagine a shot flying horizontally with this velocity, 2,640 feet per second, 20 miles high, and calculate its history before and after.

At this height, and for a considerable distance lower, air resistance may be ignored in the absence of any appreciable atmosphere, although enough, as we see, to fire the meteorite; so we begin by calculating the advance of the shot on the parabolic theory while it has risen or fallen, say, 15 miles, before or after; and is still some 5 miles high above the ground. The time occupied in each of these movements will be about 70 seconds, so that the total horizontal advance is 70 miles.

We may suppose this parabolic trajectory raised on stilts 5 miles high, out of the region of appreciable air resistance, and then draw the supporting stilts at each end as the initial and final part of a real trajectory in the resisting air, first in a free-hand sweep to give some notion of the extra range at each end. The sweep of the curve must be flatter than the parabola and outside at the gun end, but inside and more curved where the shot is falling; and these are the two parts to be examined with greater care and attention to the dynamical equations with air resistance. The effect is to reduce the angle of elevation below  $45^\circ$ ; but it is probable a greater elevation is given, say,  $50^\circ$ , or even  $55^\circ$ , to clear the lower dense strata, as pointed out in the "Text Book of Gunnery," 1904, page 225.

On the parabolic theory this would make the muzzle velocity greater than  $2,640 \sqrt{2} = 3,735$  feet per second, so we may infer that a velocity is required in the region of 4,000 feet per second. Sir Andrew Noble's gun must be lengthened again, by another 25 feet to 150 calibres, to see if it is not possible to reach and surpass this 4,000 feet per second velocity, and so produce a rival to the German long-range gun. Another increase of velocity of some 15 per cent, to 4,600 feet per second, should increase the range some 30 per cent, from 75 miles to 100 miles, and then it would be possible to bombard London from Calais, the time of flight being about 3 minutes.

In the opinion of some experts the velocity of the present gun is from 5,000 feet to 6,000 feet per second, enough, in our opinion, for a range of over 100 miles. According to French estimates the initial velocity would be about 1,400 metres per second, 4,600 feet per second; and the weight of the shell is 100 kg. to 120 kg. This is rather light for the calibre 22 cm., 9 inches, but enables the muzzle velocity to be increased, and the loss of driving power through the air is made up by fitting a long sharp-pointed Spitzer head of sheet iron, making the external appearance very similar to a modern rifle bullet  $4\frac{1}{2}$  calibres long. The actual steel shell body is only a little over 2 calibres long, and the bursting charge in the cavity is divided in two by a diaphragm, with the object apparently of preventing a premature explosion during the discharge of the gun.

First accounts described the shell as arriving in two parts, and a theory was advanced that extra ranging power was obtained by an internal explosion about mid-range. But as in the sharp-pointed rifle bullet, it is the scientific design of the exterior shape which gives the extra ranging power, with a diminished weight for high muzzle velocity.

The uncertainty in all these calculations of long-range fire is the estimate of air density in the upper strata of the atmosphere, discussed as a whole; a subject much debated in modern meteorology and geophysics.

Two assumptions are employed of this constitution, between which the true density of the air may be supposed to lie: (1) Isothermal equilibrium; (2) Convective equilibrium. These are discussed in the writer's "Hydrostatics," Chapter VII.

On the isothermal theory the temperature of the air is taken to be constant, and then the correction for the density of the air at a height  $y$  feet is the tenuity factor  $\tau = e^{-y/k}$ , where  $k$  is the height of the homogeneous atmosphere, taken at ordinary temperature at 27,000 feet, say, 5 miles, but stretching or contracting with variation of temperature like an air thermometer. At this rate  $\tau = e^{-1} = 0.368$  at 5 miles high, so that air density and resistance is a little over one-third the value at ground level, and so could not be ignored. Airmen have flown at this height, or nearly. At 10 to 20 miles high,  $\tau = e^{-2}$ ,  $e^{-4} = 0.135$ , 0.018, still appreciable, although small.

But on the convective theory the temperature is taken to diminish uniformly with the height  $y$ , according to the law  $\frac{\theta}{\theta_0} = 1 - \frac{y}{c}$ , connecting temperature absolute  $\theta$  with

ground temperature  $\theta_0$ ; and  $\frac{c}{\alpha} = \frac{\gamma}{\gamma - 1} = 3.5$ ,  $\gamma$  the ratio

of the two specific heats of air, taken as 1.4.

According to this law, the tenuity factor

$$\tau = \left(1 - \frac{y}{c}\right)^{\frac{1}{\gamma-1}} = \left(1 - \frac{y}{c}\right)^{2.5}$$

and the atmosphere ceases at a height  $y = c$ , something less than 17 miles; and before attaining this height the temperature would be so low that the air would become liquid and frozen, and the shot would have to break the ice to reach a height of 20 miles. But this is in contradiction to the Twilight effect, or the height of the Krakatoa volcanic dust observed by astronomers. We conclude then that convective theory should not be employed beyond a limited height in the atmosphere.

If these artillery ranges go on increasing at the present astounding rate, we shall soon have to abandon the old parabolic theory of Galileo with a gravity in parallel lines; but take into account the convergence to the center of the earth, and treat the shot as an independent planet by Newtonian methods, moving in the absence of air resistance in an elliptic orbit, about the center of the earth at the focus. A geometrical investigation is given in "Notes on Dynamics," 1914, page 118, taking a body projected from a Krakatoa volcano as illustration, or some of the dynamical conditions postulated in the romance of Jules Verne, as it was not anticipated we should ever see this fiction become the reality of our own time. The witty imaginary picture showing the shot that has missed London and gone round the world, in *The Bystander* July, 21 1915, will no longer appear so highly impossible. The shot is depicted grazing the surface, and returning in 90 minutes or 80 minutes if girdling the equator, according as it moved eastward or westward; not in 40 minutes as Shakespeare's Puck, a velocity which would carry him away into space, unless held down by more than fourfold gravity.

### Mineral Wealth of Alsace-Lorraine

THE German ambition is to secure the great iron basin of Briey, the richest and most extensive of all Europe, estimated to contain from three to four thousand million tons of ore. Thirty miles away Nature has deposited 10,000 million tons of coal in the basin of the Sarre; east of Nancy, partly in France and partly in Lorraine, is one of the largest salt beds in the world; farther east, at Pechelbronn, is an oil district whence 30,000 tons of mineral oil are extracted yearly; and finally, more to the south, near Mulhausen, is a wonderful deposit of potash, rivaling that of Stassfurt. Germany before the war was very poorly off for iron. In her own territory she had only 710,000,000 tons in sight. Her conquest of 1871 gave her a reserve of over two thousand million tons in Lorraine, but in spite of this she was lately obliged to import 12 million tons from Sweden, Spain, and Algeria, and was looking to Morocco to aid her supply, whence the "Agadir affair."

### Mineral Riches of the Far East

MINING engineers, who have recently visited the southern Siamese Malay States, have come to the conclusion that one of the richest mineral areas in the world is to be found there. In addition to wolfram, rich deposits of tin alluvium are found in the valleys and gullies of all the hills in which wolfram has been found, says *Eastern Engineering*. In most of the hills the number of wolfram lodes already located exceeds ten, and in all of them tin has also been found. Plenty of water with sufficient head is said to be available for washing out the tin in the rainy season, and there are possibilities for storing water in reservoirs for the dry season. There is a waterfall close by with sufficient head to develop electric power for working a large number of mines. In northern Siam, mining areas adjoining the new railway extension have been opened, and there antimony and lead are the minerals worked, the lead being mixed with zinc and containing some silver.—*The Engineer*.

Remarks on the Temperature of Space\*

By Ch. Fabry, Professor of the Faculty of Science at Marseilles

WHEN a body is exposed to a radiation it absorbs and transforms into heat at least a portion of the radiations which it receives and its temperature rises until there is an equilibrium between the energy which it absorbs and that which it loses at the same time. Other things being equal, moreover, the quantity of energy absorbed is the greater in the same ratio as the absorbing power of the receiving body is higher. This might suggest that a body with a black surface would get hotter than any other, but it is easy to perceive that this is not true. In conditions which are easy to imagine, if not to realize, it is possible to have temperatures much higher than those of a black body subjected to the same radiation. This comes from the fact that the black body, while it absorbs more heat than any other also radiates more; when absorption is only partial, but selective, and if the radiation is the sole cause of loss of energy, we may obtain very elevated temperatures of equilibrium. Let us give some results relative to a concrete example.

Let us consider a body isolated in empty space, in such manner that it can lose no energy except by its own radiation. Let us suppose that it receives the solar radiation as it is before penetrating our atmosphere; we know that the curve of energy in function of the length of wave presents its maximum in the visible spectrum toward  $0.5\mu$  and that nearly all the energy is found in the visible spectrum and the beginning of the infra-red; only an infinitesimal fraction of the energy is found beyond 2 feet. The total intensity is about 0.12 watt per square centimeter.

If the receiving body is black, it absorbs all it receives, but its own radiation increases as  $T^4$ , and the temperature of equilibrium is not very high. If we suppose the body to be spherical, and accept that its temperature becomes uniform by conductivity, and likewise suppose it placed in a vacuum and isolated in space distant from any source of radiation except the sun, we find that its temperature of equilibrium is 280 degrees absolute. The energy which it loses then is in the form of radiations of great wave length, in the neighborhood of  $8\mu$  to  $10\mu$ , so that the region of the incident radiations and that of the radiations emitted are entirely distinct.

If the surface is gray (partial, but non-selective absorption), nothing is changed. It is not the same if the absorbing power is a function of the length of wave.

While bodies absorb very slightly the visible spectrum and the beginning of the infra-red, but they have for the most part an absorbing power, and consequently an emitting power, approximating one for long wave lengths; at a slightly elevated temperature they radiate almost like a black body, but as their absorption is considerably less they do not get very hot.

The reverse is true of a body whose absorbing power has a very low value for long wave lengths, but which markedly absorbs the visible and the beginning of the infra-red. The radiation at slight temperatures is then almost nil, and though the absorption is less than in the case of the black body, the temperature of equilibrium is much higher. Such must be the case with most metals, which are almost perfect reflections for long wave lengths, but much poorer ones for short wave lengths.<sup>1</sup>

To treat the problem numerically it is necessary to know the spectral curve of the energy of the incident radiation as well as the curve of the absorbing power of the surface in function of the wave length. The following results relate to a case which is easy to treat.

The receiving body is spherical and isolated in a vacuum in space. Its absorption is supposed to be limited to a single band in the neighborhood of a wave length  $\lambda$ , i. e., the absorbing power is nil except for a group of radiations not far from  $\lambda$ . This body receives the solar radiation, which may be compared (by a hypothesis sufficiently exact, roughly speaking) to that of a black body at 6,000 degrees absolute seen under an apparent diameter of  $32'$ .

According to the value of  $\lambda$  which defines the band of absorption, we find the following values for the absolute temperature  $T$  of the body:

$\lambda$	$T$
$\infty$	0
0.4	2,000
0.5	1,700
1	1,000
2	550
5	250
10	130
Black body	280

\*L'Astronomie.

<sup>1</sup>If we desire to realize this in experiment we shall find it in conditions different from what have been supposed.

a. The presence of air causes, by convection, a loss of energy almost independent of the nature of the surface, which may suffice to lower the temperature of the metal below that of the

We see that when the band of absorption is in the region of the small  $\lambda$ , we obtain very high temperatures. A body which absorbs only the violet end of the spectrum will attain the fusion temperature of platinum by mere exposure to solar radiation outside our atmosphere. This strange result is easily explained if we remark that the violet radiations do not begin to be emitted except at a very high temperature, and that by the hypothesis the body under consideration cannot exchange energy except under the form of radiations of this kind. If the body becomes more distant from the source of radiation the temperature of equilibrium becomes lower. In the case of the black surface the absolute temperature varies in inverse ratio with the square root of the distance; if the absorption is selective the law of decrease is entirely different and may be much slower. Let us return to the spherical body isolated in space, and suppose that it recedes from the sun; let us compare what takes place in the case wherein the surface is black with the result obtained with a surface having a single band of absorption in the vicinity of  $\lambda=0.4\mu$ ; at the distance of Neptune (30 times that of the earth), the black body is at 50 degrees absolute; the selective body at 1,450 degrees. At a distance such that the sun is reduced to the apparent size of a star of the first magnitude (five years of light), the black body is at 0.4 degrees, the selective body at 830 degrees. Finally, at a distance 10 times greater yet, the sun will appear merely as a star of the sixth magnitude and its radiation alone will still maintain the selective body at 750 degrees absolute.

These results appear to be of such nature as to modify the ideas which may be conceived of the temperature of space. In a situation where there is no matter we may attempt to define the idea of temperature by introducing a test body and seeking the temperature of equilibrium. The simplest idea is to take a black body. But this case presents nothing special, since the temperature of another body may be much lower or much higher than that of the black body.

If, for example, our sun was extinguished, what would be the temperature of space in the region which we inhabit? One might calculate from certain data which are, to be sure, not very well established, that the radiation of the stars would maintain a black body at 2.6 degrees; a body absorbing only the region  $0.4\mu$  would attain 970 degrees absolute.

Is it possible that celestial bodies exist endowed with selective absorption such that they may realize the highly elevated temperatures involved by the theory? It does not appear to be absolutely impossible for gaseous masses.

Gases exercise upon light an eminently selective absorption. Let us suppose a gaseous mass, exercising an absorption, but a very feeble one, upon certain radiations of the visible spectrum, or of the ultra-violet; if the absorbing power of this gas is nil for the infra-red, and if there is no other cause of loss of energy besides thermic radiation, this gas will attain a temperature enormously higher than that of a black body placed in the same region of space. It will become heated up to the point where it will emit the only radiations it can admit, those it absorbs. This gas, therefore, possesses the property of emitting certain radiations under the influence of the same incidental luminous radiations; it is endowed with a sort of fluorescence, or rather, of re-emission, analogous to that studied by Wood and by Dunoyer, but which is in reality nothing other than a purely thermic radiation.

A case in which we may suspect the existence of such a phenomenon is that of the tail of comets. The spectrum of the bands of comets indicates emission by a gas, connected, however, with the presence of the sun, for its intensity increases when the comet approaches that star. While we are quite able to reproduce in the laboratory the various bands of this spectrum, the cause which excites this luminosity proper to comets remains to be found. An effort has been made to explain it by the action of the cathodic radiation of the sun, which has also been made responsible for the explanation of the polar auroras; but this latter phenomenon would indicate a very intermittent emission, which does not appear easy to reconcile with the phenomena observed in comets. In a manner likewise quite hypothetical we can imagine a purely thermic radiation due to a very high temperature provoked by a slight selective absorption exerted upon the solar rays.

black body. It would be well to place in a vacuum the body exposed to solar radiation.

b. Surrounding objects and the atmosphere (or the walls of the glass vase in which the vacuum is produced) behave almost like a belt at a uniform temperature; the body exposed to the sun receives, besides, the radiations of long wave length which this belt sends to it. For this reason all temperatures of equilibrium are increased, but the qualitative considerations on the influence of the absorbing properties of the surface are not modified.

We may therefore consider it as certain that if we expose to the sun bodies suspended in a balloon containing a vacuum, the bodies with a metallic surface will become much hotter than the black bodies.

Even in the air ordinary observation shows that metallic surfaces get very hot in the sun, e. g., zinc roofs.

There has been a previous attempt to compare comets to fagots of wood, which, by falling into the sun, contributed to the support its flame. The hypothesis expressed by this metaphor is not now tenable. The idea it presents with the reservations imposed by a subject so difficult, would lead us to compare these errant stars to very chilly persons, who, being obliged to traverse immense desert spaces, have become particularly skilful in the art of utilizing the slightest radiation. The farther they get from the sun the better they know how to use its rays, and where the stupid black body would be frozen, make use of these to maintain a comfortable temperature of a thousand degrees. Even in the most distant part of their course they know how to extract some heat from the feeble rays of the sun. The radiation to which they owe their beauty will be the recompense of their skill in seizing the most tenuous luminous undulations.

Proper Storage Conditions for Potatoes

OWING to the fact that a diseased condition of potatoes, known as "blackheart" is by no means uncommon, a series of investigations was instituted at the New York Agricultural Experiment Station, at Geneva, and a report on the subject contains the following statement:

"The results of this investigation emphasize the importance of providing ventilation for potatoes in storage. The need of ventilation depends very largely upon the temperature. As the temperature rises the volume of air required increases rapidly. At low temperatures, potatoes may be stored in deep piles for long periods of time. At high temperatures, it is necessary to avoid deep piling or else provide special means of ventilation. If the temperature is kept below  $40^\circ$  F. potatoes may be piled in bins and cellars to a depth of six feet without any ventilation except what provided through free access to the air overhead. Under such conditions, potatoes may be stored with safety for at least six months and perhaps longer. It is probable that no harm will result if the temperature goes up to  $45^\circ$  F. for a few days. But a long period of storage followed by a two weeks' exposure to a temperature of  $50^\circ$  F. or higher is liable to result in the ruin of most of the tubers below a depth of about three feet. Potatoes stored in deep piles should be carefully watched in the spring as the temperature rises. A few days of high temperature may cause much loss.

"It is not in the province of this bulletin to discuss the means by which the ventilation of potatoes may be secured. The methods to be employed will vary according to circumstances. The principles to be kept in mind are as follows:

- (1) Potatoes stored at high temperatures require more ventilation than those stored at low temperatures.
- (2) Better ventilation is required for potatoes which are to be stored for a long period than for those which are to be stored for only a short time.
- (3) Until more accurate determinations are made, six feet should be regarded as the maximum depth of which potatoes may be piled without special provision for ventilation when stored for six months at temperature below  $45^\circ$  F. If greater depths than six feet are employed ventilators should be provided and so arranged that none of the tubers will be more than six feet distant from an abundant supply of air.
- (4) At temperatures of  $50$ - $70^\circ$  F. potatoes should not be piled over three feet deep if they are to be kept longer than about three weeks.
- (5) No kind of ventilation is sufficient to prevent the occurrence of blackheart in potatoes kept for even a few days continuously at a temperature above  $100^\circ$  F.
- (6) Complete exclusion of the air will ruin potatoes at any temperature.
- (7) Small potato pits do not need ventilation; but some provision should be made for the ventilation of large pits."

A Miracle Explained

ANOTHER Indian "miracle" has been explained by scientific investigation. The *Pioneer Mail* of January 11th reports a lecture by Sir J. C. Bose on "The Praying Palm Tree" of Faridpur. While the temple bells call the people to evening prayer, this tree has recently been seen to bow down in prostration, and to erect its head on the following morning. Large numbers of pilgrims have been attracted to the place, and offerings to the tree are said to have been the means of effecting marvellous cures. Sir J. C. Bose first procured photographs which proved the phenomenon to be real. The next step was to devise a special apparatus to record continuously the movement of the tree by day and night. The records showed that it fell with the rise of temperature and rose with the fall. The records obtained in the case of other trees brought out the fact that all the trees are moving, each movement being due to changes in their environment.—*Nature*.

# The Pyranometer\*

## An Instrument for Measuring Sky Radiation

By C. G. Abbot and L. B. Aldrich

THE instruments we are about to describe are the result of investigations begun under a grant from the Hodgkins Fund in 1913. They are derived in principle from the highly successful pyrheliometer of K. Angström.<sup>1</sup> In that instrument there are two strips of blackened manganin, one of which is shaded from the solar radiation, the other exposed. The shaded strip is heated by an electric current whose strength can be graduated until the temperatures at the back of the two strips are equal, as shown by means of thermo-elements attached to the rear of the two strips. When the equality of temperature is brought about, as shown by zero deflection of the galvanometer, it is assumed that the energy of radiation absorbed in the exposed strip is equal to the energy of the electric current dissipated in the shaded one. To eliminate errors the uses of the strips are reversed, so that the formerly shaded strip is exposed to radiation, and the formerly exposed strip is shaded and heated by the electric current.

In another instrument of K. Angström, called by his son the pyrgeometer,<sup>2</sup> a pair of blackened manganin strips alternate with a pair of polished gold-plated ones, and the whole grid of four strips, arranged centrally nearly in the plane of the surface of a nickel-plated box, is exposed to the night sky. The bright strips lose very little heat by radiation, while the black ones lose comparatively a good deal, and so the effect is to cool the blackened strips with respect to the bright ones, and this state of affairs is indicated by means of thermo-elements attached to the series of strips. It is provided that the electric heating current can be used to warm the blackened strips until their temperature is restored to that of the bright strips as indicated by the zero of the galvanometer. This instrument was not regarded by its inventor as a primary instrument, and following his procedure the constant of such instruments is determined by exposing them within inclosures of constant-lower-temperature walls.

It might appear that the pyrgeometer could also be employed in daytime to determine the radiation scattered from sunlight by the sky, if at such times the sensitive strips were covered by a hemisphere of glass to cut off exchange of rays of long wave lengths. In such a case the heating current would require to be applied to the bright strips rather than the black ones. This use of the instrument is, however, defeated by the fact that the absorption coefficient of the bright strips for skylight is not even approximately zero, and varies greatly with the wave length, especially in the blue and violet and ultra-violet parts of the spectrum.

It was our purpose to devise a standard instrument for measuring the solar radiation scattered inward by the sky in daytime, and it was our hope that the instrument suitable for this purpose should also be applicable to the measurement of nocturnal radiation as well. We began experiments for this purpose in 1912; and now, after having devised and constructed six different forms of instrument, we satisfied ourselves that the last two types are very satisfactory for the purpose.

The name Pyranometer,<sup>3</sup> selected for the instrument we have devised, is taken from Greek words ( $\pi\epsilon\rho\alpha$ , fire;  $\acute{\alpha}\nu\alpha$ , up;  $\mu\epsilon\tau\rho\omega$ , a measure) signifying that which measures heat above. The name was chosen with reference to the fact that the instrument is designed to measure the energy of radiation to or from a complete hemisphere lying above the measuring surface.

### PYRANOMETER A. P. O. 6

Referring to the accompanying illustration, Fig. 1 is a side view; Fig. 2, looking down from above; Fig. 3, an attachment not used in measuring total sky radiation, but employed when it is desired to restrict the measurements to the sun alone; Fig. 4, a cross-section taken at right angles to the view presented in Fig. 1 and omitting the wooden base and apparatus for shading the sun. In Fig. 5 are details showing the arrangement of the sensitive

strips and thermo-couples. The instrument shown is the sixth form we have devised. In the fifth form there is but one sensitive strip instead of two as shown here. The fifth form of instrument is more sensitive than the sixth form, but has a certain source of error which was to a considerable extent avoided in the sixth form, of which more will be said hereafter.

A and B are strips of manganin, each exposing surfaces 6 mm. long and 2 mm. wide. The strips are bent through 45 degrees at the ends of the exposed portion and soldered with great care on to the lower part of the spilt copper blocks C, C', D, D', in such manner that the solder goes exactly to the bend where the manganin strip becomes exposed. The strips A, B, are situated in the center of the polished nickel-plated copper block E and are separated the one from the other by the copper strip F. Electrical insulation between the strips A, B (with their attached copper blocks C, C', D, D'), and the plate E and strip F are provided by means of thin vertical separating strips of mica, coming exactly to the common surface of the plate E and manganin strips A, B. Conductors (not shown) run from the blocks C, C', D, D', to the switch H, and thence to the pair of binding posts G, of which only one of the two appears in Fig. 1. Between the switch H and the blocks C, C', D, D', the electrical current for heating the strips A, B, divided

steady state is produced thereby is different in the two because the strip B is ten times as thick as the strip A, and so its thermal conductivity to the ends is greater. Hence a deflection of the galvanometer occurs. This deflection is balanced, after again shading the strips, by means of an electric current divided between the strips A and B so as to produce equal heating effects in each. By suitable adjustment the deflection of the galvanometer which was produced by the absorption of radiation is reproduced by the heating of the electric current. In these circumstances the energy of the electric current transformed into heat in either strip is equal to the energy of the radiation absorbed by either strip. The instrument is primarily designed to measure radiation on a horizontal surface, but it can be used in any position.

The remaining details of the instrument will be easily understood. K is an optically figured hollow hemispherical screen of ultra-violet crown glass 25 mm. in diameter and 2 mm. thick, whose purpose is to admit direct or scattered solar radiation, but to prevent the exchange of the long wave-length radiation between the manganin strips and the sky. A nickel-plated shutter, L, is provided for shading the instrument from the sun or sky. A small metal screen, M, subtending 0.0011 hemispheres, is mounted on an equatorial axis operated by a worm-wheel arrangement. This screen is used to shade the sun from the strips, in case it is desired to measure the sky alone, and not the sun and sky in combination. A nickel-plated box, N, enclosing a wood block in which lies the plate E, is provided to keep the copper plate E from external disturbances of temperature by wind currents. Around this box N fits a nickel-plated cover, O, shown in Fig. 3, for use in observing the sun alone, in making comparisons with the pyrheliometer. When the cover shown in Fig. 3 is employed, the equatorial mounting of the sun-screen M is removed, and the worm attachment is used for rotating the solar cover box O, just described.

The following data were used to determine the constant of Pyranometer A. P. O. 6:

	Cm.
Length of strips between soldered portions	0.623
Width of thin strip (mean of 5 places)	0.198
Width of thick strip (mean of 5 places)	0.201
Electrical resistance of thin strip	0.2740
Electrical resistance of thick strip	0.0369
Electrical resistance in series with thin strip	0.819
Electrical resistance in series with thick strip	0.364
Assumed absorption of the lamp black	0.98
Assumed transmission of the glass hemisphere (allowing for 2 reflections with index of refraction, 1.5)	0.92
(Thickness of strips determined by weighings approximately 0.00034 and 0.0030 centimeters).	
(Resistance of the two thermo-couples in series 30 ohms)	

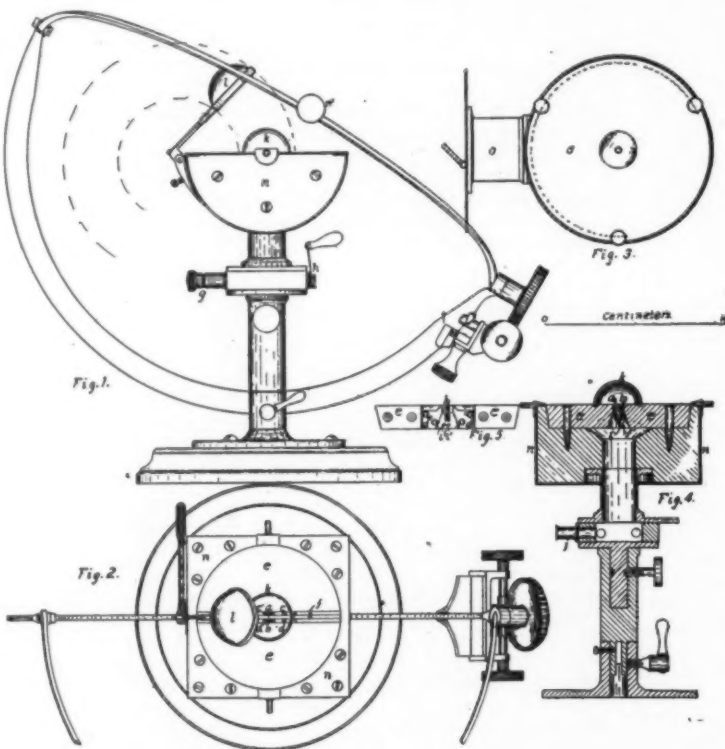
From these data the current in the thin strip is  $\frac{.401}{1.494}$  times the current in the outside electrical heating circuit. Hence the current squares are as 0.0719 to unity. Hence the constant of the instrument (when glass covered) is

$$K = \frac{.0719 \times .2740 \times 60}{4.185 \times 0.623 \times 0.198 \times 0.98 \times 0.92} = 2.54,$$

so that the energy of radiation corresponding to a given heating current C measured in amperes is 2.54 C<sup>2</sup> calories (15° C.) per cm.<sup>2</sup> per minute. If used at night without glass for measurement of long-wave rays, the constant should probably be taken at

$$2.54 \times \frac{92}{100} \times \frac{98}{95} = 2.41.$$

The reader will perceive that the instrument may be used for the sun alone, the sun and sky in combination, the sky alone by day; or by removing the glass screen K it may be used for nocturnal radiation. We have not as yet employed the instrument much for the measure-



\*From Smithsonian Misc. Coll., Vol. 66, No. 7.

<sup>1</sup>Astrophysical Journal, Vol. 9, page 332.

<sup>2</sup>Smithsonian Misc. Coll., Vol. 65, No. 3, page 28.

<sup>3</sup>We make our acknowledgments to Miss M. Moore and to Dr. Casanowicz for advice in selecting this name.

ment of nocturnal radiation nor have we as yet compared its readings under that arrangement with the radiation of enclosures at different temperatures. We hope to make such experiments in future. We have made numerous comparisons between the instrument as arranged for day observations and the pyrheliometer. A series of observations of this kind, interspersed by readings on the whole sky, is shown in table I. A close agreement with the results of Pyrheliometer A. P. O. 9 is found for all altitudes of the sun when the pyranometer readings are reduced to vertical incidence. This confirms the accuracy of the instrument for observations of the entire sky.

#### PYRANOMETER A. P. O. 5

As stated above, we employed but one sensitive strip in Pyranometer A. P. O. 5, embedding the cool junctions of the thermoelements in the copper plate V. This form is several times as sensitive as Pyranometer A. P. O. 6, so much so that we employed with it a potentiometer current to bring the very large galvanometer deflections to zero, and then balanced the potentiometer current by heating the strip. Unfortunately, a defect of this pyranometer is a secondary deflection, caused by the warming of the portion of the plate V under glass as soon as the shutter is opened. This secondary deflection was found very large, sometimes even as great as a quarter of the primary one. Its direction was sometimes in one sense, sometimes in the other, for reasons that we have not fully understood. There is, however, a method of reading whereby this source of error is very nearly eliminated. It was noticed that when heating the strip with the electric current no secondary deflection occurred, and the primary deflection was complete in 20 seconds. When heating by radiation a nearly complete temporary halt of the deflection occurred at about 20 seconds after exposure, before the secondary deflection appreciably manifested itself. Hence we balanced the radiation deflection exactly on the 20th second by the potentiometer current, closed the shutter, waited two full minutes for restored zero conditions, and then balanced the potentiometer deflection by the heating current. Under these conditions the error is practically negligible, and on account of its great sensitivity Pyranometer A. P. O. 5 is regarded as a valuable instrument.

Its constant is determined as follows: Length of strip, 0.628 cm.; width, 0.294 cm.; electrical resistance, 0.300 ohms. Radiation =  $KC^2$  where

$$K = \frac{0.300 \times 60}{4.185 \times 0.628 \times 0.294 \times 0.98 \times 0.92} = 25.9.$$

We have employed this Pyranometer A. P. O. 5 in numerous measurements of radiation from the sun, sun and sky, sky alone, and new fallen snow. In comparisons with the pyrheliometer it gave very nearly equal results when corrected to vertical incidence. The reflecting power of snow for combined sun and sky rays was found to be 70 per cent.

Some of the results found from the measurements with Pyranometer A. P. O. 5 are given in table II. We draw attention to the results on cloudy and partly cloudy days, which indicate that the sky light as a whole, on days when it is cloudy but not thick enough to rain, is of the order of two or three times the intensity of the sky light excluding the direct sun on clear days.

The pyranometer is a very handsome instrument as constructed by Mr. Kramer. It may be used readily by anyone equipped with the auxiliary apparatus used with the Angström pyrheliometer. Its readings on the sky and sun by day appear to be truly expressed in calories per square centimeter per minute, for in solar comparisons values found agree within experimental error for all zenith distances with those of our standardized pyrheliometers. We are undertaking further experiments to test its accuracy for long wave-rays such as compose nocturnal radiation. While we have hitherto employed only ultra-violet crown glass screens, it is obvious that

The secondary plate heating effect is not wholly absent from the two strip form of Pyranometer No. 6, but it is very greatly reduced in its percentage importance. To entirely eliminate it, however, we have found it necessary to close the shutter 30 seconds after opening, and then to wait at least one minute before balancing with the electric current.

such screens might be covered with stained gelatine, or other screens of special glass employed to restrict the measurements to special regions of spectrum, as might be desirable in botanical investigations. While the two strip form is preferable from its greater freedom from temperature disturbances, the single strip form is so much more sensitive that for observations in deep shade, as in a forest, it would be more suitable.

TABLE I.—Summary of Results of March 31, 1916  
North Tower, Smithsonian Institution

Sec. Z	Pyranometer A. P. O. No. 6				Pyrheliometer A. P. O. No. 9 (Calories)	Pyrheliometer A. P. O. No. 6 (Calories)
	Sky alone (Calories)	Sun and sky (Calories)	Sun alone (Calories) (x Sec. Z)	Sun alone* (Calories) (x Sec. Z)		
1.340 (A. M.)	.....	.....	.....	1.232	1.218	.988
1.330 (A. M.)	.....	.....	.....	1.084†	1.193	1.10†
1.235 (P. M.)	.1783	1.150	(1.200)	.....	(1.190)	(.991)
1.383	.....	.....	1.013	.....	.998	.984
1.400	.....	.....	.995	.....	.990	.995
1.420	.....	.....	.949	.....	.983	1.035
1.435	.....	.....	.975	.....	.987	1.011
1.485	.....	.....	.....	1.000	.993	.993
1.502	.....	.....	.....	1.019	1.020	1.001
1.545	.....	.....	.....	.947	.964	1.018
1.564	.....	.....	.....	.956	.967	1.011
1.665	.1978	.....	.....	.....	.....	.....
1.689	.1703	.....	.....	.....	.....	.....
1.730	.1757	.....	.....	.....	.....	.....
1.768	.....	.635	(.830)	.....	.....	.....
1.802	.....	.640	(.875)	.....	.....	.....
1.874	.1463	.....	.....	.....	.....	.....
1.897	.1500	.....	.....	.....	.....	.....
2.050	.....	.....	.780	.....	.775	.994
2.097	.....	.....	.798	.....	.770	.966
2.280	.1359	.....	.....	.....	.....	.....
2.338	.1359	.....	.....	.....	.....	.....
2.415	.....	.404	(.660)	.....	.....	.....
2.480	.....	.388	(.648)	.....	.....	.....
2.567	.1220	.....	.....	.....	.....	.....
2.943	.....	.....	.668	.....	.6825	1.021
3.055	.....	.....	.702	.....	.680	.969
3.280	.....	.....	.608	.....	.6325	1.040
3.420	.....	.....	.633	.....	.6493	1.024
3.760	.0851	.....	.....	.....	.....	.....
3.90	.....	.2471	(.613)	.....	.5706	(.930)
4.05	.0945	.....	.....	.....	.....	.....
4.45	.....	.....	.....	.504	.5220	1.034
General mean	.....	.....	.....	.....	.....	1.006
Omitting observations Nos. 2 and 18	.....	.....	.....	.....	.....	1.005

\* Constant of instrument different from preceding column, allowance being made for the removal of the glass.  
† Ammeter probably stuck.  
‡ Result on sun obtained by subtracting sky from sun and sky combined.

TABLE II.—Summary of Readings of February, 1916. North Tower, Smithsonian Institution, Pyranometer A. P. O. 5

Date	Sec. Z	Sky alone (Calories)	Sky and sun (Calories)	Sun alone (Calories) (x Sec. Z)*	A. P. O. 9 (Calories)	A. P. O. 5 (Calories)	Kind of sky
1916 Feb. 17	2.33	.224	.....	.....	.....	.....	Sky & cloudy.
	2.495	.274	.....	.....	.....	.....	Cloudy.
	2.84	.....	.313	.....	.....	.....	(Sun mostly hidden.)
	3.00	.165	.....	.....	.....	.....	Cloudy.
	3.10	.....	.296	.....	.....	.....	(Cirro-cumulus.)
Feb. 18	2.045 A. M.	.157	.....	.....	.....	.....	Lower 30° all cloudy.
	2.015	.210	.....	.....	.....	.....	Upper 60° all cloudy.
	1.905	.306	.....	.....	.....	.....	Lower 30° all cloudy.
	1.710 P. M.	.193	.....	.....	.....	.....	Upper 60° all cloudy. Sun shining.
	1.730	.....	.728	.....	.....	.....	Upper 60° all cloudy. Sun shining.
	1.750	.263	.....	.....	.....	.....	Upper 60° all cloudy. Sun shining.
Feb. 21	1.950 A. M.	.1060	.....	.....	.....	.....	Lower 30° all cloudy.
	1.700	.....	.800	(1.16)	.....	.....	Upper 60° all cloudy.
	1.683	.1266	.....	.....	.....	.....	Upper 60° all cloudy.
	1.667	.....	.952	(1.40)	.....	.....	Upper 60° all cloudy.
	1.570 P. M.	.1127	.....	.....	.....	.....	Upper 60° all cloudy.
	1.580	.....	.992	(1.30)	(1.38)	.998	Upper 60° all cloudy.
	1.580	.1163	.....	.....	.....	.....	Upper 60° all cloudy.
	1.593	.....	.962	(1.35)	(1.36)	1.008	Upper 60° all cloudy.
	3.60	.0780	.....	.....	.....	.....	Upper 60° all cloudy.
	3.75	.0780	.....	.....	.....	.....	Upper 60° all cloudy.
	3.95	.....	.313	(.932)	(.912)	.979	Upper 60° all cloudy.
	4.13	.....	.291	(.890)	(.883)	.992	Upper 60° all cloudy.
	4.28	.0746	.....	.....	.....	.....	Upper 60° all cloudy.
				Mean	.....	.992	Upper 60° all cloudy.

\* Values in this column obtained by interpolating for "sky alone," subtracting interpolated values from "sky and sun" and multiplying by secant Z.  
† Values in this column obtained by plotting logarithmically seven readings of pyrheliometer A. P. O. 9 made at various times during the afternoon and interpolating from this plot values to correspond with values of secant Z.

As in the case of the silver disk pyrheliometer, we are authorized to state that the Smithsonian Institution will undertake to furnish pyranometers at cost to those who are engaged in investigations which will be greatly promoted by the use of this instrument. The cost cannot yet be exactly estimated, but it will not exceed \$150 for the pyranometer itself. This does not, of course, include a galvanometer, ammeter, or batteries. Suitable slide wire resistances will be included. If desired, an

equatorial mounting additionally will be furnished at cost, so that the instrument can be used as a pyrheliometer at right angles to the solar beam.

The authors have designed and tested with satisfactory results an instrument for measuring solar and sky radiation by day and terrestrial radiation by night. Two forms of the instrument are described. Either form will be furnished at cost by the Smithsonian Institution to institutions or individuals doing important investigation which will be promoted by using the instrument.

#### Detecting Organic Impurities in Sands\*

THE important characteristics of sand for use in concrete are durability, cleanness and grading. In the following discussion we are concerned only with the question of cleanness. Experience in concrete construction and numerous tests have shown that the appearance of a sand is not a safe criterion for determining its suitability for use in concrete. For example, a sand which appears dirty may be entirely free from organic impurities and give excellent results, providing the characteristics of durability and grading are satisfactory. On the other hand, many sands which appear to be clean are coated with organic impurities of a nature that will produce very inferior concrete.

Numerous tests have been used for determining whether or not a sand possesses the requisite cleanness for use in concrete. The most common tests which have been used for this purpose are the determination of silt, and the loss in weight resulting from heating the sand to a red color. The silt test gives a measure of the amount of fine material—generally clay or loam—which is contained in the sand, but furnishes no information as to the probable effect of such materials on the strength and durability of concrete or mortar made from the sand. Experimental work carried out in the Structural Materials Research Laboratory, Lewis Institute, Chicago, have shown that it is the presence of organic impurities of a humus nature that is responsible for the effects observed from using sand of this kind. This humus material usually comes from the over-burden of soil which is found in most sand pits; it may find its way into the sand in other ways. It has been pointed out by many writers that the detrimental effect of silt in concrete is not proportional to the quantity of silt in the sand. The explanation for this result lies in the fact that it is only the impurities of an organic nature that have a decidedly injurious effect in retarding or preventing the setting and hardening of the cement; consequently, a considerable proportion of clay may be present without producing any effect other than a reduction in the strength which may be expected from the change in the grading of the aggregate.

Researches carried out in this Laboratory have shown that a simple colorimetric test may be used for detecting the presence of organic impurities of a humus nature in sands. (It is seldom that organic impurities other than those of a humus nature are found in natural sand.) This experimental work was begun through the cooperation of the Laboratory and Committee C-9 on Concrete and Concrete Aggregates of the American Society for Testing Materials.

Two methods of testing for organic impurities have been developed:

1. An approximate test for field use.
2. A more exact method for use in the laboratory.

The laboratory method differs from the field method principally in that comparison is made with definite color standards.

#### METHOD FOR FIELD TEST

The field test consists of shaking the sand thoroughly in a dilute solution of sodium hydroxide (NaOH) and observing the resultant color after the mixture has been allowed to stand for a few hours. Fill a 12-ounce graduated prescription bottle to the 4½-ounce mark with the sand to be tested. Add a 3 per cent solution of sodium hydroxide until the volume of the sand and solution, after shaking, amounts to 7 ounces. Shake thoroughly and let stand for 24 hours. Observe

\*Prof. Duff A. Abrams in the Concrete Highway Magazine.

the color of the clear liquid above the sand. A good idea of the quantity of the sand can be formed earlier than 24 hours, although this period is believed to give best results.

If the solution resulting from this treatment is colorless, or has a light yellowish color, the sand may be considered satisfactory in so far as organic impurities are concerned. On the other hand, a dark-colored solution the sand should not be used in high-grade work such as is required in roads and pavements, or in building construction. An unusually dirty sand or soil high in loam would make a sand unsuitable for use in concrete.

While it is not practicable to give exact values for the reduction in strength corresponding to the different colors of solution, the tests made thus far show this relation to be about as follows:

Color Plate Number	Reduction in Compressive Strength of 1-3 Mortar at 7 and 28 days—Per Cent
1	None
2	10-20
3	15-30
4	25-50
5	50-100

Washing sands has the effect of greatly reducing the quantity of organic impurities present. However, even after washing, sands should be examined in order to determine whether the organic impurities have been reduced to harmless proportions.

#### APPARATUS

The following list includes sufficient apparatus for making five field tests at a time:

Five 12-ounce graduated prescription bottles.

Stock of 3 per cent solution of sodium hydroxide (dissolve 1 ounce of sodium hydroxide in enough water to make 32 ounces.)

This material can be purchased at a cost of about \$1.

#### CONCLUSIONS

Experience and tests have shown that it is the presence of organic impurities of a humus nature that is responsible for most defective sands. The colorimetric test furnishes a simple and inexpensive method for detecting the presence of such impurities. The test is useful for:

1. Prospecting for sand supplies.
2. Checking the cleanness of sand received on the job.
3. Preliminary laboratory examination of sands.

This test is now being used by a large number of testing laboratories, engineers and contractors in passing on the suitability of sands for use in concrete.

In certain instances the test has been made the basis of specification requirement for sand.—*The Concrete Highway Magazine.*

## The Electroculture of Crops\*

### A Review of Important Experiments in Plant Physiology

By Ingvar Jorgensen, Cand. Phil. (Copenhagen), D.I.C., and Walter Stiles, M.A. (Cambridge)

"THE electrification of growing vegetables was first begun in Britain. Mr. Maimbray at Edinburgh electrified two myrtle trees during the whole month of October, 1746, when they put forth small branches and blossoms sooner than other shrubs of the same kind which had not been electrified. Mr. Nollet, hearing of this experiment, was encouraged to try it himself."

This quotation from Joseph Priestley's *History and Present State of Electricity*, published in 1767, records the simple experiment which formed the introduction of the subject of electroculture, and which was soon to be followed by numerous other experiments, which were repeated again and again at intervals, and are still being repeated in our time.

The subject is one the development of which through its various phases is particularly interesting to follow, not because of the achievements of the investigators therein, but on account of the light that is thrown on the factors making for success or failure in the solution of scientific problems.

It is instructive in the light of our present knowledge to take a survey of the field of investigation as it is presented by the works of Priestley and his contemporaries, who worked and wrote when experimental science was still in its infancy. It is interesting to note the variety of observations made by these "philosophers" who were engaged in this preliminary survey of natural phenomena. To us who live in the age of specialization in study this method of attack may appear strange and wasteful, but it is rather astonishing to realize the acuteness of the observational powers of these philosophers, by whose efforts were thus collected a large number of elementary observations, many of which formed the nucleus for enormous development. And the early method of investigation may not have been so wasteful after all, for we know how, today, fresh fields of investigation are opened out by new combinations of subjects.

In reviewing, then, the course of investigations in the time of Priestley, we find that the subject of electroculture was as favorite a one for examination as other branches of electrical science. We cannot avoid asking ourselves, therefore, how it is that while the study of electricity and its many industrial applications has developed into enormous importance, electroculture in the meantime has remained practically stationary for a century and a half, and this in spite of its obvious economic importance.

We probably find the answer to this question in the stagnation of the science of the living plant. The development of electroculture depends not only on the development of our knowledge of pure physics, but also on the development of our knowledge of the activities of the plant. While physics has developed so rapidly, the science of the living plant remains very much where it was when Woodward and Stephen Hales performed their experiments. While some excuse for this may be found in the political and economic conditions which have determined the position of agriculture, the main reason for this state of affairs must be that the science of the living plant has not attracted the genius which has been bestowed on electrical science for instance.

For the sake of simplicity we shall only deal in this article with the form of electroculture in which electricity is discharged through the air to the plants from an overhead wire system, kept charged at a high potential by an electrical machine, or simply charged by atmospheric electricity collected at a higher altitude. This is the only form of apparatus for electroculture which has been employed on anything like a commercial scale,

\*From *Science Progress*.

although very numerous experiments have been also made by passing currents through the soil in which the experimental plants are growing.

The subject, even with the restriction which we have indicated in the last paragraph, has such an enormous literature that we shall only attempt to cite the work of a few investigators typical of the various periods in which they made their observations. From these typical investigations we shall see how it appears to be manifest that electricity in certain cases exercises a remarkable influence on plant life, but that, on the other hand, many observers, with apparently as much justification, insist that electricity, if it has any influence at all on plant growth, has a harmful influence. The same discrepancy appears both in the case of small- and large-scale crop experiments. We shall attempt to correlate this discrepancy with other plant physiological investigations and quote parallel examples from recent electroculture research, and thus we hope to impress on the reader that this discrepancy is probably not due to a fault on one or other side of the authors of these discordant observations, but to a lack of realization on both sides of the exact position of the problem—a lack of knowledge of the life of the living plant, and a lack of knowledge of the experimental conditions of the electric discharge employed. This discrepancy, which has lasted for a century and a half, is not likely to be removed before a changed outlook is brought about, but as soon as this results it seems likely that out of all this apparently futile research on electroculture may arise a knowledge useful to mankind. This is likely to be the case not only in the particular example of electroculture, but in all that concerns stimulation of plants.

The first detailed description of experiments comes from the Abbé Nollet, the French court physicist, who, hearing of Maimbray's experiments, tried some experiments himself in the following year, 1747. Nollet filled two pewter vessels with similar samples of earth, and in the two vessels equal quantities of mustard seed were sown. After two days one of the vessels was subjected to the influence of the electric discharge for about ten hours—namely, from 7 a. m. to 12 noon, and 3 to 8 p. m. The other vessel was kept as control in the same room and at the same temperature, Nollet pointing out the usefulness of M. Réaumur's invention for measuring temperature. The next day both vessels were exposed to the sun. On the day following three seeds had germinated in the electrified vessel and produced seedlings three lines high; in the non-electrified control no seeds had germinated. The experimental vessel was again electrified in the evening, for three consecutive hours; the next morning it was found to contain nine seedlings seven to eight lines high. At this time still no germinations had taken place in the control vessel, although towards evening the first seedling appeared in it. In the afternoon of this same day the experimental vessel was again electrified for five hours, and electrification was continued up to the eighth day. At that time all the seeds in the electrified vessel had germinated and were fifteen to sixteen lines high, while in the control vessel only two or three seedlings had appeared, and these were no more than three to four lines high.

Similar observations on the stimulation by means of electricity of the plant in various stages of its life—germination, the growing period, opening of dormant buds—were made by numerous observers at this period, for instance Jallabert, Menon, and Nürnberg.

About this time atmospheric electricity had become a favorite subject of investigation, and we find suggestions to the effect that atmospheric electricity is an important

environmental factor in the life of the plant. Thus Father Beccaria of the University of Turin, writing in 1775, says: "With regard to atmospheric electricity it appears manifest that nature makes an extensive use of it for promoting vegetation"; and again: "Besides, the mild electricity by excess (positive electric action of low tension), which, as I have observed for these many years past, constantly prevails when the weather is serene, certainly contributes to promote vegetation, in the same manner as experiments have shown us that this is likewise the effect of artificial electricity without sparks. And is it not likely that the former kind of electricity promotes vegetation still better than the latter can do, since nature increases it and lessens it in such circumstances and at such times as particularly require it?" Similar views were put forward at the same time, and with perhaps even greater force, by the Abbé Bertholon, who designed an apparatus, the electro-vegetometer, for collecting atmospheric electricity and distributing it over growing crops.

There were, however, a few observers who concluded from their experiments that electricity was either harmful to vegetation, or at least did not stimulate it. The earliest of these appears to be Koestlin, who in 1775 reported that negative electricity is harmful to vegetation; but probably it was the testimony of the famous Dutch physicist Ingen-Housz which carried most weight, and who on account of his reputation as a plant physiologist, has been often quoted in favor of the theory that stimulation of vegetation by electrical means is impossible. However, as Ingen-Housz connected his plants directly with the collectors, and he describes the plants as shrivelling up after treatment, his experiments simply indicate the possibility of killing plants by electricity.

In 1789, the year following Ingen-Housz's more elaborate experiments, D'Ormoy found that the electric discharge stimulated the germination of mustard (and lettuce) seed. This, it will be observed, was Nollet's original experiment.

The few experiments we have cited of the many performed in this first period of half a century are not only typical of the eighteenth century, but we find the same repeated in the nineteenth century; the experiments differ only in the people who perform them, in the means of producing the discharge, and in their being usually conducted on a larger scale; but in all we find the same absence of any realization of the actual position of the problem.

We find that a lively interest in the action of atmospheric electricity on the plant had developed in this country before the end of the first half of the nineteenth century. Thus Forster in 1844 reported the results of some experiments with chevalier barley in which atmospheric electricity was collected by a horizontal wire and conducted to the soil by vertical wires at either end of the horizontal one. Forster found the electrified crop assumed a darker green color and grew more rapidly than the non-electrified control, while the yield of grain from the electrified plot was double and the yield of straw triple that of an average crop. In 1846 William Sturgeon, lecturer at the Manchester Institute of Natural and Experimental Science, went so far as to say that Forster's experiments "have commenced a new era in electro-cultural enquiries; and their flattering results have induced several persons, electricians and others, to try the same plan on crops of various kinds of the last year's growth." However, it was no new era that was inaugurated by Forster's experiments; it was simply a repetition of older experiments that resulted, the methods employed often being less refined than those of

earlier workers. Nevertheless, Sturgeon's paper on the Electroculture of Farm Crops, published in the *Journal of the Highland and Agricultural Society* for March, 1846, contains some very trenchant remarks on the position of the subject. At that time the importance of manuring was coming into great prominence as a result of Liebig's researches and writings, yet Sturgeon realized that, in spite of the obvious benefits that were resulting from the application of pure chemistry to the questions of soil and plant a greater knowledge of the life-activities of the plant would be required before agriculture could develop to its fullest extent. As this writer has been completely neglected, we feel justified in giving one or two quotations from his paper. He says, for instance: "By what powers, or by what physical forces, do the organs of plants display, and keep in operation, their respective functions of vegetable life, is a problem of vast importance in the basement of agricultural science, and in every other branch of vegetable culture. This grand problem, the solution of which has not yet been accomplished, nor, indeed, scarcely attempted, presents the most formidable, and, at the same time, the most noble bulwark yet to be assailed in our inquiries respecting the functions of vitality in the vegetable kingdom." And again: "The rules of his art will always enable the practical chemist to be of much service in providing food for plants, although it may require a higher order of investigations than those he is in the habit of pursuing to discover the character and operations of those forces which stimulate the organs of vegetables to avail themselves of the food thus supplied for their use."

However, in spite of these laudable views put forward in 1846, the subsequent history of electroculture is little else than a repetition of the earlier. An enormous number of researches have been conducted on the subject, but the vast majority are on exactly the same lines as the older ones, and the results are similar, i. e., the majority show favorable influence on germination, growth, and final yield resulting from electrification, a minority show no such improvement resulting. No leading principles are brought out and no contributions from other branches of science throw light on the subject.

It was during this period that the science of the physiology of plants was obtaining a certain amount of recognition, but unfortunately instead of developing along its own lines as the science of the living plant, and evolving its own guiding principles, it became subservient on the one hand to morphological botany, on the other to chemistry, with the result that although much has become known about individual processes in isolated organs over short periods of time, and still more conjectured, we know next to nothing about the interrelation of the various processes which make up the life of the plant and their variation in different phases of that life. It is no wonder, therefore, that the scientific agriculturist has always been very sceptical of the results obtained in plant physiology, and that there has resulted an apparent discrepancy between the results obtained in agricultural practice and in physiological experiments.

Hence it is not surprising that in regard to the contributions of plant physiology to electroculture there is very little to be said. In all the physiological researches conducted in order to solve problems of electroculture there is a lack of realization of the electrical problems involved and a neglect to inquire into the progress of pure physics, so that there is a general idea that it is unessential in these experiments to trouble about the conditions of discharge of electricity, and that results obtained, for instance, from experiments with currents of low E.M.F. through germinating seeds can be used as arguments in regard to experiments with electric discharge through air on actively growing and maturing plants.

A favorite subject of investigation by the physiologists was the effect of electricity on *protoplasmic movement*. In 1837 Amici and in 1838 Becquerel and Dutrochet studied the influence of an electric current on the movement of protoplasm of *Chara vulgaris*. Similar experiments were conducted at a later date by Heidenhain (1863), Kuhn (1864), and others, on protoplasmic movements in the leaves of *Vallisneria*. Similar experiments have been performed up to our own day, but nothing fruitful for our subject has resulted from them.

A few investigators have attacked the problem from the point of view of determining whether the beneficial effect of electroculture is due to an acceleration of the *assimilatory process*. For this purpose a current was passed through a piece of a water plant, as by Thouvenin (1896), or through an aerial leaf, as by Pollacci (1907), and in each case an increased rate of assimilation was recorded as a result of the passage of the current. But these experiments are open to so much criticism that it is impossible to draw any conclusions from them. This holds also for arguments derived from *in vitro* experiments, such as those of W. Lob (1905), who argues from the supposed formation of formaldehyde from carbon

dioxide and water under the action of the silent discharge, to the increase in crop production resulting from the electric discharge.

It was proposed by Gassner in 1909 that the beneficial effects so often observed as a result of the electric discharge are chiefly due to an influence on the *transpiration rate*. This writer observed that more than twice as much water was transpired by the plants subjected to the electric discharge than the non-electrified control plants. He suggests that this is simply brought about by the formation of air currents by the silent electric discharge, and these alone would be sufficient to explain the increase in evaporation. However, the general criticism we have levelled against all the physiological experiments dealing with electroculture hold for these as for all the others. But that the water relations of the plant are influenced by the electric discharge is an opinion fairly generally held. Thus Nollet was struck with the more rapid rate of evaporation from electrified liquids than from non-electrified.

The *respiration* of plants under various electrical conditions has formed the subject of an investigation by Knight and Priestley published in *Annals of Botany* for 1913. However, these various electrical conditions are not those of the actual electrocultural experiments in the field, and the results of many of the experiments of these authors, as they themselves admit, are of dubious interpretation on account of the experimental arrangement.

Recently a paper appeared by R. Stoppel in which the cause of the *movements* of the leaflets of *Phaseolus multiflorus* was traced down to atmospheric electrical changes, and the author from this observation proceeds to far-reaching generalizations as to the importance of atmospheric electricity in the life of the plant.

We see, therefore, that the physiological investigations in reference to electroculture have dealt with numerous plant processes: assimilation, transpiration, respiration, irritability, protoplasmic movement. In no one case have the experiments been conducted in such a way that they give us any information as to the influence of any definite electrical conditions on any one of these processes at any definite stage in the history of any plant or any plant organ. How much farther off are they therefore from even suggesting a solution of the problems of electroculture?

Exactly similar criticism can be levelled against those who regard the benefit resulting to growing crops from the electric discharge as due to changes brought about in the soil; with this school it is therefore not necessary for us to deal further.

It is strange that the greatest and most remarkable contribution to electroculture should have come from a physicist, namely S. Lemström, who was Professor of Physics in the University of Helsingfors. His work is remarkable not only because it was the first in which comparatively large areas of land under crops were subjected to treatment, but because he pursued the subject with great energy through his lifetime right up to his death in 1905. He carried out experiments in many countries under a great variety of different conditions, and so collected a great deal of empirical information as to the conditions under which the discharge affected the life of the plant. Further, he made many experiments with the object of discovering the manner in which the discharge affected the plant, but in this matter his lack of knowledge regarding the life of the plant prevented him from expressing himself in a way which would appeal to plant physiologists or scientific agriculturists. Nevertheless, it is likely that some of his observations and ideas may prove to be sound when correlated with later experiments. His researches make it clear that the overhead electric discharge will affect the life of the plant in all its phases: germination, vegetative growth, and maturation. Lemström in his experiments used the unidirectional discharge from an influence machine, but he obtained favorable results when either the positive pole or the negative pole of the influence machine was connected to the overhead network. Similarly he was able to obtain favorable results whether the discharge was applied in the daytime or at night. He sums up his experience, however, by stating that the best results are obtained (1) with the network positively charged; (2) by applying the discharge in the morning and the evening; and (3) by having the general conditions favorable for plant growth.

Although Lemström used an influence machine giving a very high potential, the overhead network in his experiments was only charged to a few thousand volts or even less. That neither he nor subsequent investigators should have derived advantage from this observation for the construction of more efficient apparatus is at least surprising. All experimenters appear to have been obsessed with the idea that extremely high voltages are necessary, while at the same time in the construction of apparatus they neglect arranging for sufficient output of current.

It is unfortunate that none of the physicists who associate their names with electroculture investigations should have taken the trouble to work out the physical questions involved. It was left to a pure physicist, working from quite another point of view, to put forward the considerations which enable us to formulate the conditions of the discharge (see J. S. Townsend, *Phil. Mag.* 1914).

That the publication early in this century of Lemström's book on "Electricity in Agriculture and Horticulture," translated into several languages, should have given rise to a large number of fresh experiments is not surprising. It is unfortunate that neither plant physiology nor scientific agriculture should have been sufficiently advanced at this time to give their contribution to the subject. As a result recent development has consisted almost entirely in the application of all possible devices for the production of high-tension current which electrical industry has evolved for other purposes. However, a certain amount of empirical information has been collected, for instance, in this country by Mr. J. E. Newman, Mr. William Low, and Miss E. C. Dudgeon, but our knowledge of the subject is not greater than when Lemström left it, in spite of improvements in apparatus.

We hope we have made it clear in the foregoing that the history of electroculture presents an ever-recurring cycle of experiments having as their object the proof or disproof that the electric discharge has a beneficial effect on vegetation. That both these results should be obtained regularly in the cycle should be sufficient to inform us that there must be something fundamentally wrong in the method of inquiry. This, in our opinion, is to be found in the neglect of quantitative measurement of the discharge, and in the lack of knowledge of the science of the living plant. This may perhaps be more easily understood if we give a few details of a parallel series of investigations by H. Molisch, published in 1912.

Molisch was concerned with the influence of radium emanation on plant life. We find in Molisch's work a realization of the principles of the application of stimuli, that the effect depends on the intensity of the agent and the length of time during which it is applied; further, that the effect of the stimulus may first appear a considerable time after the application. Molisch gives evidence that he possesses a good knowledge of the living plant; thus he realizes that the same agents react quite differently on a plant in its resting period, in its period of germination, and during its active growth.

Molisch, therefore, from his first empirical experiments which showed that radium emanations would induce growth in a resting organ (flower-buds) in winter time, does not conclude that radium emanation will always have a beneficial influence on plant life. He determines by a carefully planned series of experiments that a definite quantity of radium emanation applied for a definite period will induce the opening of flower-buds; but if the organ is not in its resting condition, application of the emanation makes no difference in the rate of development. Further series of experiments deal with the action of various quantities of emanation applied over definite times, to germinating seeds or growing seedlings. It was generally found in these cases that a much smaller quantity of emanation than that applied to resting organs has a distinctly harmful effect and hinders development, but in the case of certain species, with extremely small quantities of emanation a slightly increased rate of growth was observed.

It is not surprising that such a series of experiments should have given rise all over the world to experiments on the value of radio-active manure, but it is remarkable that some of the investigators, in whose experiments an arbitrary and unknown quantity of agent was used and which was allowed to act throughout the whole season, should argue from the negative results obtained by them that radium emanation is without influence on plant life, and Molisch's experiments are consequently disproved.

There thus seems to be the possibility of the production of a cycle of experiments, dealing with the use of radium emanation, similar to that which already obtains in regard to electroculture, and this in spite of Molisch's original well-planned work.

If we now take in review the electroculture experiments and consider them in the light of the work on radium emanation with which we have just dealt, we find that all investigators show the same lack of realization (1) of the necessity for quantitative measurement of the electric discharge, (2) that a stimulus may act differently on the plant at different stages of its life, (3) that the effect of the stimulus depends on its intensity, (4) that the effect of the stimulus depends on the time for which it is applied, and (5) that the effect of the stimulus may appear a considerable time after it is applied. Numerous examples of this can be quoted. J

That in the electric discharge we possess a means which can be used as stimulus at most periods of plant life seems clear from the greater number of experiments, but how indifferent, or even hostile, plant physiologists have been to the general conception of stimuli is perhaps best illustrated from some experiments of Gassner on electroculture published in 1907. First of all he finds that with *Pisum* and *Helianthus* the electric discharge has no influence on germination and growth, the electrical treatment lasting eight to fourteen days, and being used for fourteen hours daily. The electric discharge was so strong that light phenomena appeared round the plants and the plants were black on account of precipitated dust. But with barley he concludes that the discharge has a favorable influence. We may quote in detail one of his experiments on this plant. The electrified series consisted of three pots where the distances between the discharging points and the plants were respectively 10 cms., 21 cms., and 35 cms. On March 12 thirty barley grains were sown in each pot. On March 16 the seedlings began to appear, and on the same day the discharge was started. On March 17 all the seedlings had appeared; no difference was visible between any of the experimental pots, nor between experimental and control. On March 18 the electrified were visibly in advance of the control and more in advance the smaller the distance between the discharging point and the plant. On that day the first leaf had appeared in 16 plants in the pots 10 cms. from the discharging point, 12 in the pot 21 cms. from the point, and 4 in the pot 35 cms. away; the numbers for the two controls were 1 and 3. In the further course of the experiment the electrified developed quicker than the control, but soon the plants farther away from the discharging point were ahead of the others. On March 27 the experiment was stopped.

Consideration of Molisch's experiments will render these observations readily understandable. It is clear that Gassner was dealing with a phenomenon of the same type as Molisch, in which the intensity of the agent and duration of its action determine the result. It is regrettable that such a view should not have presented itself to Gassner and that he should simply have attempted to explain his experimental results in the light of a single process, namely, evaporation of water.

Limitation of space has prevented us in this article from going into detail in regard to the very numerous experiments which have been performed on electroculture. We hope, nevertheless, we have made clear our main object, which is to emphasize that the condition for development of electroculture is a sounder outlook in plant physiology.

It is true that in the past the contributions of plant physiology to our subject have been negligible, and what advances have been made have come from the physicist. Nevertheless, although a knowledge of physical methods is essential for intelligent research in electroculture, the problems involved are essentially problems of plant physiology. With a sounder outlook in plant physiology, the apparent discrepancy between plant physiology and agriculture should disappear, and great developments in the methods of crop production should take place. It is only when research, based on this new outlook in plant physiology, has become established, that we shall be able to judge how far the application of stimuli, including the electric discharge, may be of economic importance in the production of crops.

### The Standardized Infant

DOGMATIC teaching is recognized as a useful method of introducing students to a subject which is new to them, and represents an economy of effort on the part of both teacher and pupil. It has its place even in regard to subjects in a state of flux, where any standard set up must be an artificial and temporary one. Such standardization is more proper on the teaching than on the research side of any study, and it may, if unchecked, lead in the long run to stagnation and to want of progress in directions where change is the inevitable accompaniment of growth. In the campaign on behalf of the health of women and children, successfully waged by the Royal New Zealand Health Society for many years past, standardization of method has proved fruitful of great results. Initiated by Dr. F. Truby King at the time when Lord Plunket was governor of the Dominion, and carried through with infinite patience over a period of discouragement, the scheme now includes 35 nurses whose general training of three years or more has been supplemented in some cases by a further year of maternity work and in all cases by three months or more of special study at the Karitane-Harris Baby Hospital. These Plunket nurses carry out over the whole area of New Zealand a series of health recommendations which in course of time have crystallized into three pamphlets, written by Dr. Truby King and issued by the Public Health Department, on the expectant mother, the baby in the first month, and the feeding and care of infants

in general. Personal work of this kind has been assisted by wholesale cooperation on the part of the New Zealand press which prints some 200,000 copies weekly, under the heading of "Our Babies' Column," of instructive matter supplied to it by the Health Society. Coming from such a successful and comprehensive scheme of dogmatic teaching, Dr. Truby King is now paying a welcome visit to this country in the hope of contributing to a similar health campaign, especially in the direction of forming a general consensus of opinion in regard to elementary matters of physiology and pathology of infant life. He has undertaken the charge of a clinic placed at his disposal by St. Thomas's Hospital. It is true that the National League for Health, Maternity and Child-Welfare has issued from its office a booklet in pamphlet form dealing with matters of maternal and infant hygiene, but the recommendations contained therein are of a general character, arrived at in the spirit of compromise, or in much the same way as the highest common factor is obtained from a group of large numbers. Dr. Truby King uses a percentage whey mixture which has become the standard method of artificial infant feeding at the Antipodes, and involves the use of lactose, which in this country is now both scarce and costly. Here and in America there is still an almost infinite divergence of practice, which may, of course, be evidence that the one ideal method of artificial infant feeding has not yet been found, but it is, at all events, a testimony to the infant's power of adaptation to his environment amounting to little less than genius. To take a single example, in London the infant whose parents cannot afford to give him fat in any of the usually approved forms has shown his capacity to digest and assimilate linseed oil in suitable emulsion, although the Bradford baby is stated to rebel at the substitute. The necessity of adding lactose to the milk mixture in artificial feeding has long been disproved by the experience that any form of sugar is equally well assimilated by the London infant if given a little time for the necessary adaptation. To adopt in London the humanization of artificial milk on the New Zealand plan would therefore, appear to be retrograde and costly, a fact to set against the advantages of a simple dogmatic scheme of teaching infant welfare workers. The successful breeding of calves and pigs on a large scale is undoubtedly expedited by the adoption of routine methods, but we have seen during the last few months many changes dictated by war conditions to which the youthful cattle have accommodated themselves with a good grace. It is, at all events, arguable whether the human infant should be standardized, and the more the important points involved are examined the better for our knowledge, assuming, of course, that only those speak who have personal experience of the subject.—*The Lancet*.

### High Flying in Modern Aeroplanes

THE most marked development in the modern machine is its extraordinary capacity for climbing to a great height in a short time. At the beginning of the war the average height flown on active service was 4,000-5,000 feet, simply because few of the machines then in use with the impedimenta carried could get much higher. Today a height of 20,000 feet is, I believe, on certain occasions reached, and it is fairly certain that if progress continues at its present rate, heights a great deal beyond this figure will be reached as a usual thing. These great altitudes bring forward many difficulties which will have to be seriously considered. The first trouble in the winter will be the extreme cold to which the occupants will be subjected unless they are protected by special cowling. This, to a certain extent, is the natural advantage obtained in the tractor. The question of the difference in the comfort of machines in this respect was shown to me in a very marked manner last winter. I was testing the fall-off of engine power at a height on a tractor two-seater in which it was specially arranged that the warm air from the radiator and engine passed along the fuselage to the pilot and then to the passenger, and although at a height of over 21,000 feet with the thermometer below freezing at ground level, I did not suffer in the least from the cold, neither did my passenger who sat behind complain until we shut off to descend. As a contrast to this, a few days later I was on a single-seater scout at an altitude of 17,000 feet, and although it was a tractor with a rotary motor, I suffered intensely from the cold, and became so numbed that my vitality must have been something akin to a dormouse under the snow, and, in spite of being well gloved, I had frostbitten finger tips which pained for many days afterwards. Surely this is a very inefficient state for a pilot at the front to have to take on an air fight or other exacting work? Put two pilots up to a great altitude, one kept well warmed and comfortable, the other half dead with the cold, and it would be easy to surmise which would be most likely to do the best work.

I really believe it is more by accident than design that the pilot or passenger has benefited at all in the past from

the heat of the engine, with the exception perhaps of the late S. F. Cody's machine. He purposely placed the radiator of his pusher in front of the pilot to keep him warm. I know from my experience when flying in France in the cold weather that the discomfort owing to the extreme cold became intense when flying only at 6,000 feet on a two or three hours' reconnaissance flight. This is a point to which designers should give attention, as machines are now capable of reaching great heights.

Cold also affects the motor pretty seriously. This is more noticeable with the water-cooled type. Unless some provision is made for blanketing the radiator surface at heights, it becomes far too cold for efficient running. Cases are known of the freezing of the water system on a descent from a great height, with pretty serious results to the motor, as well as the difficulty of getting the engine to run again efficiently through being too cold to effect a landing. In the future war machine the pilot must have a very wide range of control over the water-cooling system.—An abstract from a paper before the Aeronautical Society of Great Britain, by CAPT. B. C. HUCKS.

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